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WITH PLATE XII.

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TABLES *VIII. An Alphabetical Index and Price-List; IX. Classified List giving Name, Composition and Form of all known Mineral Species and Varieties, with Supplement; X. A Complete Metallic Classification of Minerals, showing the Natural Combinations of the Metals.*

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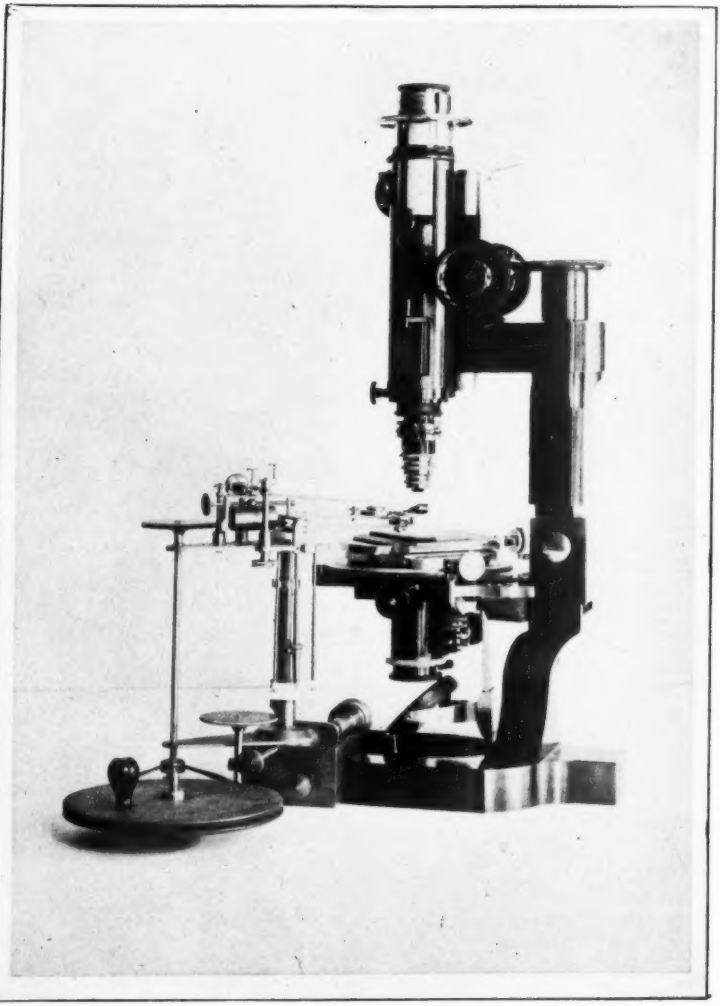


THE MICROSCLEROMETER.

THE

AMERICAN JOURNAL OF SCIENCE

[FOURTH SERIES.]

ART. XLII.—*A Microsclerometer, for determining the Hardness of Minerals;** by T. A. JAGGAR, JR., Cambridge, Mass. (With Plate XII.)

Introduction: the definition of "hardness."

THE hardness of minerals and metals has been investigated by the following methods:

ABRASION-TESTS.

Scratching by hand: (Werner, Haüy, Mohs, Breithaupt, Cohen†, etc.)

Drawing mineral under a point:

(*a*) H. = weight on point— (Seebeck, Franz, Grailich and Pekarek,‡ Exner§).
(*b*) H. = weight to draw mineral (inversely)—(Grailich and Pekarek).
(*c*) H. = number of abrading movements—(Grailich and Pekarek).

* This research has been carried on in the petrographical laboratory of Harvard University, Cambridge, Mass.; the author is much indebted to Professor J. E. Wolff, Dr. Charles Palache, Mr. L. W. Page and Professor Victor Goldschmidt, of Heidelberg, for advice and assistance. For the admirable mechanical execution of the instrument, as well as for many valuable suggestions, I have to thank Mr. Sven Nelson, of Cambridge, to whose skill as an instrument maker no words of mine can do justice.

† v. Rosenbusch, Mikros. Physiographie, I, p. 258.

‡ Sitzungsber. d. k. k. Akad. Wien, xiii. 1854 (contains complete bibliography of earlier papers).

§ Preisschrift, Wien, 1873.

Drawing point over mineral:

(*a*) H. = weight to draw point (inversely)—(Franz).
(*b*) H. = weight on point—(Turner*).

Grooving with a standard edge:

H. = depth of groove—(Pfaff†).

Boring with a standard point:

H. = number of rotations—(Pfaff‡).

Grinding with a standard powder:

(*a*) H. = period required for polish (inversely)—(Behrens§).

(*b*) H. = loss of volume (inversely)—(Rosiwal‖).

(*c*) H. = comparative loss of four substances (inversely) (Jannettaz and Goldberg¶).

STATIC PRESSURE-TESTS.

Compressing lens on plate of substance:

(*a*) H. = limit pressure per unit of surface—(Hertz**).
(*b*) H. = " " " " multiplied by the cube root of the radius of curvature—(Auerbach††).

Compressing a point into a surface:

(*a*) H. = weight to reach a standard depth—(Calvert and Johnson‡‡, Bottone§§).

(*b*) H. = volume of indentation (inversely)—(U. S. Ordnance Tests‖‖).

Reference to the foregoing table shows a wide diversity of method, all designed to measure the resistance which a substance opposes to permanent deformation; all come within the scope of four processes utilized as the measure of such deformation, viz:

(1) Abrasion, (2) Penetration, (3) Friction, (4) Fracture.

Of the eighteen authors mentioned, thirteen used abrasion (76 per cent); friction was used as an alternative (and found inadequate) by two of these. All were mineralogists except Turner, who was a practical metallurgist. The five authors who used static pressure as a test of hardness (penetration and fracture) were physicists and metallurgists, and in all five cases

* Proc. Phil. Soc., Birmingham, v, 1886. † Sitz. k. k. Bayer. Akad., 1883.
‡ Sitz. k. k. Bayer Akad., 1884.
§ Anleitung zur Mikrochemische Analyse, 1895.
‖ Verhandl. k. k. Geol. Reichsanstalt, 1896, xvii, 475.
¶ Assoç. Franç. p. l'Avanc. d. Sc., 9 Aug., 1895.
** Verhandl. Berlin Phys. Gesells, 1882. †† Wied. Ann., 1891, 1892, 1896.
‡‡ Phil. Mag., xvii, 114. §§ Chem. News, 1873, xxvii.
‖‖ Report of experiments on metal for cannon, U. S. Ordnance Dep't, 1856.

objection has been made to the results obtained on the ground of the interference of tenacity and plasticity. Dana has stated concisely the generally accepted definition of the mineralogists: "Hardness is the resistance offered by a smooth surface to abrasion."

For practical purposes it has been demonstrated that when softer substances are abraded by a very hard substance, other conditions being constant, the amount of abrasion suffered varies with the hardness of the abraded substance. For obtaining values relative to an empirically selected abrader, it is obvious that any one of the numerous variable conditions in the process may be selected for functional purposes, provided all the other conditions are maintained constant. What is required, then, is an absolutely defined abrading agent, a method, one condition, variable with the resistance to abrasion, selected as functional, and devices for maintaining the others absolutely constant. The values obtained would be relative to the hardness of the abrader: this is a point that seems to have been frequently overlooked. Attention has been called, in the text-books,* to the fact that the results of sclerometry show the hardness differences between the lower members of the Mohs scale to be much less than between the harder members, where the differences are said to be enormous, as shown by the great length of time requisite for polishing the harder gems, etc. It must be remembered, however, that when the diamond scratches quartz there is interaction, the phenomenon of the scratch is a relative one; if sufficient force is applied the diamond may be perceptibly abraded as well as the quartz. On the basis of the definition that hardness is the resistance to abrasion by diamond, diamond is almost infinitely harder; but assuming hardness to be the molecular or atomic tenacity, we must have a means for measuring that tenacity in absolute terms of energy expended before we may be sure of the actual differences between different substances.

The hardness of a substance as expressed in resistance to scratching, is, in the case of fine-granular aggregates, dependent on the fineness of the particles, the interlocking or loose structure, and the strength of the cement. If such an aggregate as, for instance, chalk is scraped with a tool, we overcome the tenacity of the particles and produce a scratch: if the substance were an aggregate of diamond particles the scratch would still be produced. If the particles are so rigidly interlocked that the tool does not overcome their tenacity, a scratch will not be produced by reason of a separation of the fragmental particles: yet we know that the soft carbonate will be scratched, and the hard carbon will resist. The first case was

* Tschermak, p. 139.

echanical structure, the second atomic structure. Again, if a mass of amorphous carbon of low specific gravity is heated in a crucible, crystalline graphite needles are formed, of somewhat higher specific gravity and hardness; if enclosed in a globule of molten iron, which expands suddenly on solidification, and so subjects its inclusions to great atomic pressures, Moissan* has shown that the carbon crystallizes in the form of carbonado and diamond, of very high specific gravity and hardness. The substance in this case is a single element throughout, its atom has retained a constant weight but the molecule has continuously increased in weight under pressure, hence the number of atoms in the molecule has increased with the hardness. This accords with the determinations by Bottone† that the hardness of metals varies as the specific gravity divided by the atomic weight, which quotient, as Turner‡ has pointed out, varies directly as the number of atoms in a unit space.

Atomic structure in the element; atomic and molecular structure in the crystalline compound; atomic, molecular and sub-microscopic structure in the amorphous or crypto-crystalline substances; atomic, molecular (sub-microscopic) and mechanical structures in the crystalline or fragmental aggregates:—all of these influence hardness as measured by physical abrasion tests; hence for comparative results of scientific value it is important that definite crystals or wholly amorphous substances be selected. If this is done, abrasion tests, delicately conducted, may give very valuable data concerning the intimate structure of solids: Exner§ has shown very interesting relations between the directions of resistance to abrasion and crystalline form: Pfaff‖ has barely touched upon a wide field of research in his experiments on the mean hardness variations of minerals in isomorphous series, and the relation of the cohesion constants to the other physical properties. And, finally, as an aid to the differentiation and determination of the crystalline minerals, there is no reason why a thoroughly defined method may not give very constant results.

Former instruments have had three chief sources of error: (1) personal variability due to using "visibility" as determinant; (2) inequalities of mineral surface; (3) undefined details of instrument. To eliminate (1), the depth of abrasion should be definite and measurable: to eliminate (2), the surface should be artificial and defined, and the boring method, where only a very small portion of the surface is initially touched, should be used: this, at the same time, gives a mean value for all directions in the surface; to eliminate (3), every part of the

* Comptes Rendus, 1894 and 1895.
† Loc. cit. ‡ Loc. cit. § Loc. cit. ‖ Sitz. k. k. Bayer. Akad., 1884, p. 255.

instrument, including the abrader, should be minutely defined, and, for comparative determinations, an empirical standard should be adopted.

The Microsclerometer.

The author's object in the present research is to describe an instrument so precisely adjusted as to eliminate these earlier sources of error. The quality which it is proposed to measure is the resistance opposed by a body to the removal of particles of its substance by a defined diamond point, moving in contact with it under uniform conditions. The instrument is applied to the microscope, so that it may be used for either thin sections or crystal faces; it is believed that the tests described will eventually be of determinative value in petrography. The adjustments of the instrument are such that any of the variable elements in the process of abrasion may be made functional while the others are maintained constant; but the best movement for obtaining a mean value is the rotary movement described by Pfaff, the number of rotations of the boring point indicating the hardness of the substance relative to the abrading point.

The instrument is shown in Plate XII, adjusted to the large Fuess microscope No. 1. Fig. 1 shows the plan, and fig. 2 the vertical elevation. The principle of the instrument is as follows: A diamond point of constant dimensions is rotated on an oriented mineral section under uniform rate of rotation and uniform weight to a uniform depth. The number of rotations of the point, a measure of the duration of the abrasion, varies as the resistance of the mineral to abrasion by diamond: this is the property measured. The instrument consists of the following parts:

(1) A standard and apparatus for adjusting to microscope.
  (*a*) Foot adjustments, (*b*) Rotating adjustments,
  (*c*) Lifting adjustments, (*d*) Fixing adjustments.
(2) A balance beam and its yoke.
(3) A rotary diamond in its end.
(4) Apparatus for rotating uniformly.
(5) " " recording rotations.
(6) " " locking and releasing.
(7) " " recording depth.

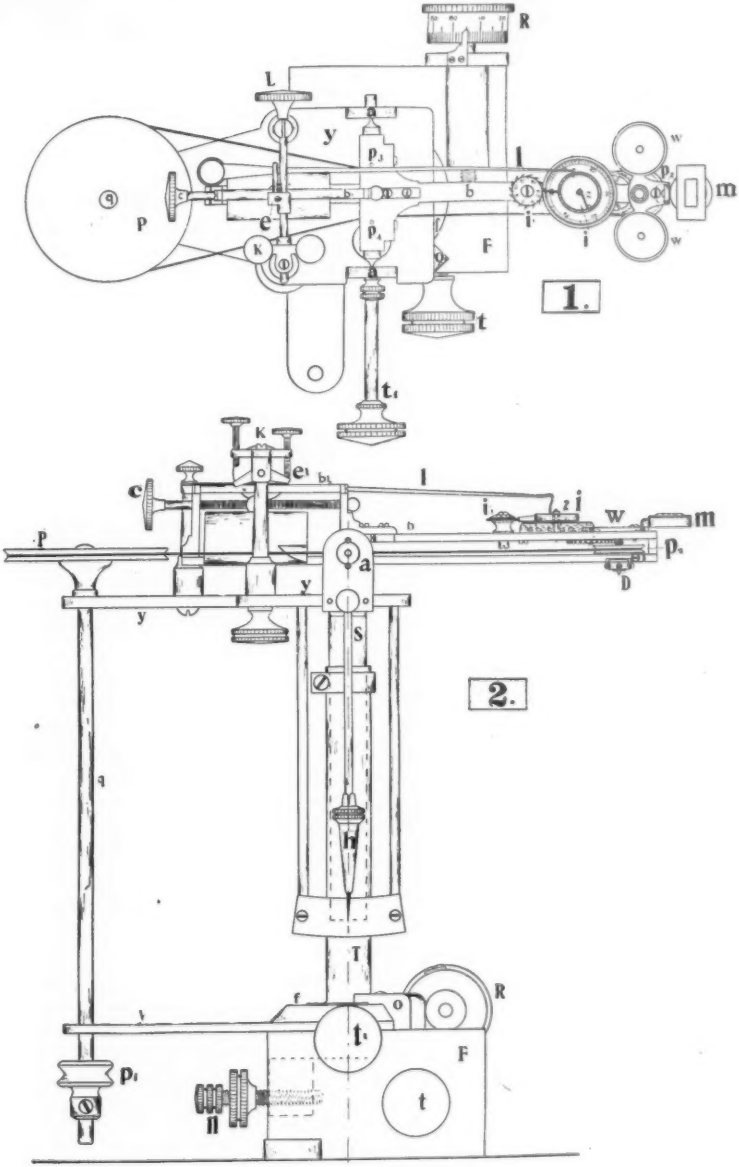
The instrument described admits of measurement with any one of the four variables, rate, weight, depth or duration. The last has been found most practical, because it gives the highest values and hence admits of the most delicate gradation.

The Standard.—The yoke *y* is supported on a brass column, which slides in an outer tube, and may be raised or lowered by

a worm at the side. The foot-block F fits the left prong of the U-shaped microscope foot. The distance to which it may be slid on this prong is limited by a set-screw *n*, and a second thumb-screw *t* binds it in place. The outer tube T, with all the apparatus which it supports, rotates on the surface of the foot-block F, and this rotation may be given fine adjustment by the graduated barrel and screw R which presses against a projection *o* from the foot plate *f* of the outer tube. The vertical rotation-axle passing through F may be made rigid in any position by the thumbscrew t_1. A V-shaped extension of the foot-plate *f*, holds the sleeve in which rotates the main pulley-bearing shaft *q*. The small pulley at the lower end of this shaft is connected by a belt with the power, represented in Plate XII as a disc and crank for rotation by hand. In the author's latest experiments a clockwork motor has been used.

The Balance Beam.—The beam, *b*, pivoted at *a*, is counterpoised by a weight adjustable by the screw, *c*. The beam consists of an upper and lower lath of brass, the space between being destined to receive the pulley system for rotating the diamond. The indicator arm bears an adjustable pendular counterpoise *h* at its lower end, to counteract the effect of the "top-hamper" on the beam. This arm passes through a slot in the lower plate of the yoke, *y*.

The Diamond.—The diamond used is a cleavage tetrahedron with a perfect point. The cleavage tetrahedron may be obtained in duplicate in great perfection among the "cleavings" used for making diamond cutters and pencils. The tetrahedra are especially valued for their pure points. As each tetrahedron has four points, it is not difficult to find a point of perfect form among relatively few specimens. The points are turned upward in the field of the microscope successively, under a high power (No. 7) until one is found which shows the three edges to the uppermost focus of the instrument, converging to a perfect point; the diamond selected is centered in its brass mount by soldering it in first roughly, and then turning down the brass in a jeweler's lathe about the diamond point as a center. The diamond D thus mounted is held in a chuck by three radial centering screws. This chuck is attached to a vertical steel spindle which passes through the pulley, p_2, and its pinion, and terminates in a jewel bearing against a smooth diamond plate in the upper lath of the beam, just under the point midway between the two weight pans, *w*. The pans are separated in this fashion and two weights used, in order to leave space for the microscope objective, which, as will be seen, is focussed down close to the micrometer scale, *m*. The superficial angle made by the two edges of each face of the diamond at its point was measured in the microscope by



Figures 1 and 2.

focus levelling in the ball-and-socket clip,* followed by rotation after centering the point to the cross-hairs: the angle in each of the three faces was found to be 62°—showing that each face is not a perfect equilateral triangle but possesses a slight bulge or rounding.

Apparatus for rotating.—To the upper end of the shaft, *q*, is attached a larger pulley, P: the shaft may be raised or lowered freely in the lower sleeve with the rest of the apparatus. The pulley, p_2, on the diamond spindle, is connected by a fine silk belt with *p:* the tension of the belt is prevented from interfering with the action of the balance by two small pulleys, p_3, p_4, rotating within the beam exactly in the pivot axis, and separated by a distance equal to the diameter of p_2. The belt tension from without is thus applied to the balance system in its axis of rotation, and thus only has the effect of adding to the sensitiveness of the balance by assuming a portion of the weight which otherwise would rest wholly on the pivot bearings. Torsional strains are obviated by the parallelism of the two portions of the belt between p_3 and p_2, and p_4 and p_2.

Apparatus for recording rotations.—The rotations of the diamond spindle are reduced 1/60 by two pinions and two gear wheels of the ordinary watch pattern. Over the second gear is a dial divided into sixtieths: the spindle from this wheel passes through a sleeve in a brass indicator plate, *i*, which acts as a hand: in this a steel spring, *z*, presses on the spindle, forming a friction bearing. The outer border of this plate carries a crown milling, against which presses the bent extremity of the spring lever, *l*. When *l* is pressed down it engages with the milled edge and the spindle rotates within the sleeve without rotating the indicator, *i;* when *l* is released, the tension of the spring, *z*, on the spindle is sufficient to cause this to rotate *i*, and each division of the dial indicates one complete revolution of the diamond. A graduated toothed wheel, i_1, is rotated intermittently on each complete rotation of *i* by the latter's index arm, recording the sixties up to fifteen revolutions—making a total of 900 revolutions of the diamond possible without renewed observations.

Apparatus for locking and releasing.—The locking apparatus affects the balance and the recording apparatus simultaneously. A half rotation of *L* causes an eccentric, *e*, to press upon the bar, *b*, above the counterpoise, and at the same time to press down the spring lever, *l*, and thus check the indicator, *i*. The adjustment of the lock is regulated by the thumbscrews, *k*, which limit the rotation of *L* by checking an anglepiece attached to the L shaft.

* See this Journal, III, 1897, p. 129.

Apparatus for recording depth.—To the end of the beam on its upper side is pivoted horizontally a ring which may be inclined about the symmetry axis of the beam. In this fits a circular plate, *m*, bearing a transparent Zeiss micrometer glass (5^{mm} divided into 100 parts). The plate, *m*, is rotary in the ring, so that the micrometer scale may be turned in any azimuth. This device is so adjusted that the micrometer scale is visible in the field of the microscope at the point exactly 10^{mm} from the axis of rotation of the diamond point: this is one-sixth of the distance from the diamond axis to the beam pivots, *a*, hence any downward movement at the diamond point is magnified by 1/6 at the micrometer. Hence if the microscope is focussed on the micrometer, before and after boring with the diamond, the depth so measured by the fine adjustment screw of the microscope will be 7/6 of the actual depth bored. If, now, *m* be rotated until the micrometer scale stands at right angles to the beam, and be then tipped gently, an inclination may be found where, under a high power, only one line of the micrometer scale is in focus at a time, and a downward focus of precisely $\cdot 01^{mm}$ or 10 microns (micromillimeters) is necessary to bring the next lower line on the slope into focus. Conversely, if we focus on the lower line and allow the diamond to bore its way down 10μ, the next higher line of the micrometer glass will come into sharp focus only when that depth is reached. We thus have here an extremely sensitive measure of depth.

Method of using the apparatus. — The standard is first adjusted to the microscope foot, so that the diamond is exactly 10^{mm} from the center of the stage (cross-hairs) as recorded on the stage micrometer scale movement. The preparation is fixed in the angle piece and clips on the stage. The beam being in perfect balance, it is locked: record is made of the readings of i, i_1 and R. Equal weights are placed on each pan. The mineral is centered to the cross-hairs of the microscope, and a part of the mineral surface is selected for the test under the low power, No. 2, whose long focus prevents the objective from striking *m*. By the stage movement the mineral is pushed 10^{mm}, when it is exactly under the diamond. The latter is lowered to the mineral surface until slight contact is indicated by the movement of the indicator hand: it is checked exactly at the point of contact—if anything a little above it. The lock, *L*, is released so that the diamond is actually resting on the mineral, and the micrometer scale, *m*, is focussed under objective (No. 7) of the microscope, so that the lowermost of two lines near the center of the field are in focus, with the inclination arranged 10μ to the scale division: ocular No. 3 is most effectual, giving considerable spherical aberration which

allows only the one line to be in sharp focus at a time. L is locked, and the rotation started with the clockwork at a uniform speed. L is released, and the diamond begins to bore. The uniformity of the rate is indicated by the movement of the index hand, i: care must be taken to note the initial position of i, and to keep record of the number of times it makes a complete revolution. The micrometer focus is watched carefully in the microscope and when the upper line appears sharp the lock is closed and the clockwork then checked. In this way the diamond is moving at a uniform rate when it begins to bore and when checked. i and i_1 now give the hardness in terms of rotations of the diamond.

The accessory parts of the apparatus are a levelling table of seasoned mahogany for the microscope with a sunk spirit level and three levelling screws, and the clockwork, which is attached to the levelling table by an adjustable slide for regulating the belt tension.

The adjustments necessary prior to experiment are:

Levelling table,
Adjusting tension of belts,
" " pivots,
" lock,
" foot-screws,
" counterpoises,
" inclination of micrometer,
Centering diamond.

The diamond is centered by inverting the apparatus and holding it in a vise so that the diamond is turned upward in the field of the microscope. With a jeweler's screw-driver the three radial screws of the chuck may be so adjusted that the extreme tip of the diamond rotates on the cross-hairs of the ocular without eccentricity: or, if it is desirable that the diamond move in a circular path, and so describe a ring shaped scratch, it may be so adjusted.

Calibration and Measurement.

The instrument is calibrated by testing the constancy of each element, viz: depth, rate, the diamond point and weight. Tests with various weights show that the action of the diamond is slow, and moreover, there is a very rapid increment of resistance with increased depth. A weight of 10^{gm}, rate 10 revolutions per second, gave the value for an ordinary cover-glass of about three thousand revolutions for a depth of $\cdot 01^{mm}$. Thus the test occupied about five minutes. The increment of resistance implies increment of surface of abrasion. The constancy

of this increment for different diamond points is now being investigated. In a future publication the calibration of the instrument will be described in detail; in the present work we shall describe only a preliminary series of tests with the minerals of the Mohs scale, in order to show the efficacy of the method.

The Mohs scale; sclerometric values.—For these tests the diamond point was adjusted slightly out of center, so that it would abrade away a perfect ring-shaped groove and thus avoid the clogging of the hole noticed in Pfaff's experiments. The micrometer scale m was arranged parallel to a cross-hair set in the 45° position from lower left quadrant to upper right, and the inclination of the scale was adjusted so that the two scale divisions nearest the center of the cross-hairs should record a difference of focal depth of 10μ. It is important that these focal measurements be read always on the same part of the field of the ocular, as there is considerable variation in different parts due to aberration. It is also necessary to adopt a uniform criterion of focal perfection; for the author a fine granular structure observed in the micrometer glass at the side of each scale division afforded a very sharp determinant, accurate in every case to a fraction of the value $\cdot 001^{mm}$ or 1μ. The rate adopted for this series of tests was 6–7 revolutions of the diamond per second, regulated by the governor on the clockwork and by the tension of the belts; this was checked at various times throughout the series and was found to maintain a very uniform average; the clockwork was wound up to its maximum tension at the beginning of each test, and in the case of the harder minerals at the end of every ten minutes, this being determined as the period through which the mainspring would maintain a uniform rate without appreciable change. The weight used was ten grams, which was found too high for the soft minerals and too low for the very hard ones, showing the advisability of using two sets and reducing to a common unit as in Pfaff's tests: the constant weight was retained, however, in the following series, in order to discover the possibilities of the instrument, and no great value is placed upon these preliminary results. Many inaccuracies will be discovered in the results attained, especially in the case of the soft minerals, which are averaged in each case from three observations, and these show considerable diversity for the same mineral, depending upon variations in surface texture, and irregularities in the rotation induced by too great weight and irregular resistance, as will be mentioned. The indices i and i_1 were read before each test and the number of complete revolutions of i_1 was recorded during the experiment. A preliminary test with soft glass showed the radius of eccen-

tricity of the diamond point to be ·08mm, with the filings most abundantly heaped outside the periphery of the ring, and a smaller ridge within.

2. Gypsum. Cleavage.
Weight 10 gm.
Depth 10μ
Rate 6·5 rev. per sec.
Hardness (revolutions) 8·3.

Under such weight the diamond penetrated to the glass under the first thin section used almost instantly: a cleavage fragment was tried, and the above value is an average of borings to several different depths, it being found impossible to stop the boring at exactly 10μ depth, so great was the oscillation of the scale *m* in the field of the microscope: this was caused by the eccentricity of the diamond, the point penetrating too rapidly and meeting unequal resistances in different cleavage directions. This difficulty was less noticeable in the hard minerals.

3. Calcite. Cleavage face.
H.... 50.

In both gypsum and calcite the groove formed was somewhat elliptical, instead of circular. This is due to diverse cohesion values for different cleavage directions as worked out by Exner. The initial variations in resistance force the diamond out of its normally circular path. Three tests on the same calcite face showed these elliptical grooves to be *similarly oriented* with reference to the cleavage fissures, the longer axis of the ellipse occupying an oblique position between the long and short diagonals of the rhomb face. This coincides with the determination by Franz of maximum and minimum hardness in the azimuth of the short diagonal in opposite directions.

4. Fluorite. Octahedral cleavage.
H.... 143.

The depth was checked by direct focal measurement in a series of observations on fluorite, and was found to conform within 0·5μ with the record given by the inclined micrometer *m*. One groove had sub-elliptical form, with a slight flattening on one side. A series of observations to test the increment of resistance with increased depth gave the following values:

Depth in μ.	Revolutions.	Rev. for each 5μ.
10	148	74
15	273	125
20	459	186
25	666	207

5. Apatite. Basal.
H .... 233.

The value is much affected by surface texture. The filings have a very mealy consistency, and seem to have a lubricating action on the work of the abrader; when the abrasion was continued for a second 10μ depth, the increment of resistance was enormous. The groove was elliptical, with the longer axis parallel to the next adjacent prism face.

6. Orthoclase. P Cleavage.
H .... 4665.

Action very constant, with uniform results for several tests. Ring nearly circular. Oscillation so slight that the micrometer focus could be observed during the rotation. It was found advisable to check the rotation with the clockwork rather than with the lock for occasional precise observation of the location of the sharp focus, as in this way the diamond was not lifted from the groove, but remained at exactly the depth attained.

7. Quartz. Basal.
H .... 7648.

Practically no vibration, very constant.

8. Topaz. Basal.
H .... 28867.

9. Corundum. Rhombohedral cleavage.
H .... 188808.

With so slight a weight the duration of this test with corundum was nearly nine hours. Hence the advisability of using greater weight with the harder minerals: nearly all the minerals of petrographic importance come into this category. Reducing the foregoing values to the standard adopted by Rosiwal, making corundum equal to 1000, we obtain the following results; the values obtained by Rosiwal and Pfaff are appended for comparison:

	Pfaff, 1884.	Rosiwal, 1892.	Jaggar, 1897.
9. Corundum	1000	1000	1000
8. Topaz	459	138	152
7. Quartz	254	149	40
6. Orthoclase	191	28·7	25
5. Apatite	53·5	6·20	1·23
4. Fluorite	37·3	4·70	·75
3. Calcite	15·3	2·68	·26
2. Gypsum	12·03	·34	·04

In addition to the determination of hardness, the microsclerometer may be used for very delicate determinations of

the thickness of mineral thin-sections; by making contact with the upper surface of a mineral and then with the object glass level at its side we may measure thickness for the Chaulnes method of determining the index of refraction. Tests with hypersthene suggested further that by boring through a mineral of high double refraction to the glass beneath we may rapidly get a value for the amount of double refraction or $\gamma - a$. The conical depression shows on its border the color rings marking various thicknesses; if we bore to red of the first order, make a depth reading, and then bore through to the glass and read again, the difference in depth gives the thickness of the mineral for red of the first order. If we do this on a section cut in the plane of the optic axes, so that the axis of mean elasticity, b, lies in the plane of polarization of the microscope, we have a thickness value which will give directly, by reference to the calculated tables (v. Rosenbusch) the category to which the mineral belongs. Lastly, the use of this instrument in treating by actual contact the individual minerals of a rock section, suggests the possibility of an adaptation by which perhaps chemical tests may be applied directly to the dust in the boring.

ART. XLIII.—*Recent Observations on European Dinosaurs;** by O. C. MARSH.

DURING the past summer, it was my privilege to attend the International Congress of Geologists at St. Petersburg, as an official delegate from this country, and this gave me an opportunity to see a number of museums and collections in Europe which I had not before visited. I thus had the privilege of inspecting personally many interesting reptilian remains that I had not previously known, and of examining others which were more or less familiar to me from figures and descriptions.

In the present paper, I have only time to speak of the Dinosaurs, in which I have long taken a special interest, and have endeavored to study all the known specimens of importance, both in this country and in Europe, having in view the preparation of a series of memoirs on the different groups of this subclass of extinct Reptilia.

London.

I began my investigations in the British Museum in London, a great treasure-house for fossil reptiles, to which I have long made frequent pilgrimages. This time the Dinosaurs were seen to better advantage than ever before, but of new or unknown forms I found that few had been added to the collection since my visit two years ago; and I consoled myself with the other extinct Reptilia, and especially with the new fossil birds and mammals from South America.

St. Petersburg.

In St. Petersburg I hoped to find many Dinosaurian remains, as here had been brought together an abundance of fossil treasures from various parts of the Russian Empire, which I knew must contain many forms of this group. In the four principal museums of the city, however, I could find no bones of Dinosaurs on exhibition, nor could I learn from any of the museum authorities that such remains had been recognized among the specimens received, neither could I find any such fossils myself among the debris of the collections, so often a rich repository for new or inconspicuous specimens. This was true, also, of the smaller collections visited, and I was at last forced to admit that here, at least, the Dinosaurs of Russia, like the snakes of Ireland, were conspicuous only by their absence.

* Abstract of Communication made to the National Academy of Sciences, Boston Meeting, November 16th, 1897.

Moscow.

This opinion was not changed by a visit to the rich geological collections of Moscow, which I examined with care; although other fossil vertebrates, including many reptiles, were abundantly represented. I was assured, moreover, by various Russian paleontologists, that in other museums of the empire or in the known localities they had seen no Dinosaurian remains. This vain quest, however, only proves that the discoveries are yet to be made, and I confidently expect them at no distant day, since in almost every other part of the world Dinosauria have already been brought to light. In northern Europe west of Russia, and in North America to the east, these reptiles were especially abundant, and the vast territory intervening must contain numerous Dinosaurs, including many new forms of the group.

Vienna.

In Vienna I knew that my friend Professor Suess had a large collection of Dinosaurs in his museum to show me, and I spent several days there in their investigation. This collection was of special interest to me, as it was from the Gosau fresh-water deposits, which, as a student, years ago, I explored mainly in the expectation of finding Cretaceous mammals; and I was not without hope of still detecting such remains during my present visit, as here were the localities where they were, in my judgment, most likely to be found in Europe. The Dinosaurs I examined were from Neue Welt in this formation, and were of great interest. They had all been studied by Bunzel, Seeley, and others, who had recognized ten or twelve distinct genera and many species among them. I could find, however, not more than a quarter of this number, and among these I found no indications of the *Ceratopsia*, which from the published figures and descriptions I supposed to be represented in this collection. The Dinosaurs with dermal armor which I saw, all pertained to the *Stegosauria*, and two distinct genera among them were more nearly like *Scelidosaurus* of the English Jura, and *Nodosaurus* of the American Cretaceous, than any others with which I am familiar. This collection contained the only Dinosaurian remains I could find in Vienna.

Munich.

I next went to Munich, which, under Professor von Zittel, has become a great center for paleontology. I found that the gem of the collection is still the unique *Compsognathus*, which in several previous visits I had studied with care. A reëxamination impressed me even more with the fact, that this is one of the most perfect and interesting vertebrate fossils yet discovered, and no other example of the genus is known. It was

in this unique specimen that years before I had detected the embryo, and this fossil still affords the only known evidence that Dinosaurs were viviparous. I could find no other Dinosaurian bones of interest in the Munich collection, the new features being mainly numerous fine specimens of *Mosasauria* from America, and some interesting remains of *Hesperornis* and *Baptornis* from the same horizon in Kansas.

I was much pleased to see here the new Jurassic fossils collected by Nansen in 1896, at Cape Flora, in Franz Josef Land. These interesting remains are now under investigation by Dr. J. F. Pompeckj, assistant in the Munich museum. I could detect no vertebrate fossils among them, although various indications favor their presence in this fauna.

Paris.

My limited sojourn in Paris gave me no opportunity for a careful examination of the museums there, but I could learn of no recent additions of Dinosaurian remains since my last visit two years before.

Caen.

I next went to Caen, in Normandy, to see the famous Dinosaur *Poikilopleuron*, so well described by Deslongchamps many years ago. Through the kindness of my friend Professor A. Bigot, I had a good opportunity to study this unique specimen, which of late has been regarded as identical with the *Megalosaurus* of Buckland, the first genus of Dinosaurs described, and one about which little is yet known.

Among the undetermined material of this museum, I was greatly pleased to find the genus *Pleurocœlus* well represented by characteristic fossils, and from a well-defined Jurassic horizon in the vicinity of Havre. The species appears to be a new one, somewhat smaller than *Pleurocœlus suffosus* from the Kimmeridge of Swindon, England. It resembled still more closely *Pleurocœlus nanus*, which I have described from the Potomac formation of Maryland.

Pleurocœlus is one of the most characteristic genera of the Sauropodous *Dinosauria*, and its value in marking a geological horizon should therefore have considerable weight. It is now known from the two European localities mentioned above, both in strata of undoubted Jurassic age. The same genus is well represented in the Potomac deposits of Maryland, and has been found, also, in the Atlantosaurus beds of Wyoming, thus offering, with the associated fossils, strong testimony that the American and European localities are in the same general horizon of the upper Jurassic.

Havre.

The last day at my disposal before sailing for America, I spent in Havre, in the Muséum d'Histoire Naturelle, where the director, M. Lennier, showed me many vertebrate fossils of interest, from the well-known localities near the city. Here again, among the fragmentary specimens not yet investigated, I found the bones of another Dinosaur, also one of the *Sauropoda*, but considerably larger than the *Pleurocœlus* at Caen. The remains were very similar to those of *Morosaurus*, and the horizon was in the Kimmeridge, which is here well defined.

From Havre, I crossed the Channel to Southampton, and with a parting look at the Wealden cliffs of the Isle of Wight, which have furnished the remains of so many interesting Dinosaurs, I sailed for home.

Yale University, New Haven, Conn., November 13, 1897.

ART. XLIV—*On the Sapphires from Montana, with special reference to those from Yogo Gulch in Fergus County;* by GEORGE F. KUNZ.

THE existence of sapphires in the State of Montana has been known for some years past, and has attracted considerable attention. Several localities are now known and several distinct modes of occurrence. They were first found in transported gravels along the bars of the Upper Missouri; then they have been found in the earthy product of decomposed dikes, and lastly farther down in the unaltered igneous rock itself; the succession thus presents a close parallel to the history of the diamond-workings in South Africa.

The first published description of the Montana sapphires was by the late Dr. J. Lawrence Smith, in this Journal (III, vol. vi, p. 185, September, 1873). He there said: "These pebbles are found on the Missouri River near its source, about sixty-one miles above Benton; they are obtained from bars on the river, of which there are some four or five within a few miles of each other. Considerable gold is found on these bars, it having been brought down the river and lodged there; and the bars are now being worked for gold. The corundum is scattered through the gravel (which is about five feet deep) upon the rock bed. Occasionally it is found in the gravel and upon the rock bed in the gulches, from forty to fifty feet below the surface, but it is very rare in such localities."

A fuller account of the conditions and yield was given by the author in his volume on "Gems and Precious Stones of North America," published in 1890 (pp. 48, 49); he subsequently visited the locality and examined it carefully, publishing the results in the Appendix to the same work (pp. 340, 342).

In 1891 the first serious attention began to be paid to the mining of sapphires in this district. The bars consist of an auriferous glacial gravel; and in working them for gold, sapphires were obtained as a by-product. By 1890 companies began to be formed and claims taken up and examined with a view to sapphire-mining. The region extends for some six miles along the Missouri River, the central point being Spokane Bar, twelve miles east of the city of Helena. Other names, such as Emerald Bar, Ruby Bar, French Bar, Eldorado Bar, etc., were given to different points of the area. The gravel rests on a slaty bed-rock and the author found minerals besides gold and sapphires; among these are small crystals of white topaz, garnets in rounded grains often of rich

color and miscalled rubies, cyanite, stream-tin, chalcedony, limonite pseudomorphs after pyrite nodules, etc. At Ruby Bar two facts of great significance were encountered, bearing on the age of the gravel and the source of the gems. The writer saw and measured a mastodon tusk three feet long, embedded in the sapphire layer of the gravel; and a dike was found cutting the slaty bed-rock beneath; in this dike were crystals of sapphire, pyrope and sanidin. All these facts were described by the writer in the Mineralogical Magazine (vol. ix, p. 396, 1891), together with an account of the rock by H. Miers (loc. cit.), who characterized it as a "vesicular mica-augite-andesite," abounding in brown mica and porphyritic crystals of augite, with a ground-mass of feldspar microlites and brown glassy interstitial matter, with magnetite.

Two years before, indeed, in 1889, the writer had seen some specimens of a trachytic rock, enclosing well-defined crystals of sapphire similar to those of Eldorado Bar, from a dike somewhat farther up the river. These facts, which were referred to in the "Gems and Precious Stones of North America" (p. 49), and the Appendix (p. 341), sufficiently showed the source of the gems as coming from the erosion of dikes of igneous rock.

More recently sapphires have been found throughout a considerable district lying some seventy-five to a hundred miles east of the Missouri bars, the principal point being Yogo Gulch, on the Yogo fork of Judith River near its headwaters, in Fergus County, Montana, on the eastern slope of the Little-Belt Mountains. The nearest town is Utica, fifteen miles to the northwest, in the same county. The sapphires occur over a somewhat extended area, which is being explored and laid out in claims. They are imbedded in a yellow earthy material, from which they may be washed out by sluicing, as for gold, the heavy crystals gathering at the bottom. Mr. S. S. Hobson, of Great Falls, Montana, the original discoverer of the gems at Yogo Gulch, states that at that point there are two veins (dikes?) containing sapphires, which have been traced for a distance of seventy-five hundred to eight thousand feet in an east-and-west course, about eight hundred feet apart. One of these is seventy-five feet wide, and consists of a "yellow earth" (i. e. completely decomposed). It has been found that what was supposed to be the end of the "vein" is really a fifty-foot fault, and that the vein can be traced very much farther. In working down to a greater depth, the unaltered igneous rock has been reached, and its full description is given in an accompanying article by Prof. L. V. Pirsson.

These Yogo Gulch sapphires have been referred to by the writer in the 16th and 17th Annual Reports of the U. S. Geo-

logical Survey, in a chapter on The Production of Precious Stones,—especially in No. 17, for 1895, p. 909; they will be further described in No. 18, for 1896.

Other localities are also coming to light in the same State; one of these is at Rock Creek, Granite County, thirty miles from Phillipsburg, where the gems are reported of good blue color, with other tints, and some pale rubies; another is on Cottonwood Creek, eighteen miles from Deer Lodge,—the stones being of varied colors, red, pink, yellow, and occasionally blue; the third has been recently announced in Choteau County.

As regards the gems themselves, marked differences appear in those from the two principal Montana regions. All are of small size, but they differ in crystallization. Those from the Missouri gravels are characterized by the presence of the prismatic faces, with the basal plane, and rarely any of the rhombohedral modifications,—the prevailing forms being hexagonal, either prismatic or so short as to be tabular. A beautiful example of this type is figured in "Gems and Precious Stones of North America" (colored Plate I, fig. C). The specimens from the minor localities have generally a similar type of form. The Yogo Gulch crystals, on the other hand, are largely rhombohedral, with the basal plane more or less present, but the prismatic and pyramidal faces hardly at all. The rhombohedron x, which is prominent in these crystals, as shown in the figures and descriptions of Dr. J. H. Pratt, has the remarkable interest of being new to this species. Other very noticeable features which the writer was the first to observe and point out, are the striations on the basal plane parallel to its intersections with the rhombohedron, and sometimes rising into steps as the oscillation becomes a replacement, as well shown in Dr. Pratt's figures (Figs. 11*a*–14*a*, p. 427), and the singular depressions on the basal plane in other crystals (Figs. 5–10, p. 425), their sides being formed by faces of the inverse rhombohedron, sometimes meeting in a point, and at other times truncated and floored by a basal plane.

We have here two distinct types of crystallization in the same mineral, from the same State, and produced apparently under similar conditions in igneous rocks. It will be extremely interesting to learn, by further exploitation and study, whether these two types bear any fixed or definite relation to the particular variety of eruptive rock in which they occur. The accounts thus far given of the rocks examined seem to suggest such a possibility.

As to the value of the early Montana sapphires in jewelry, it is hardly possible yet to predict how far it may be really important. Much beautiful material has already been obtained,

but little of high value. Those from the Missouri bars had a wide range of color,—light blue, blue-green, green, and pink, of great delicacy and brilliancy, but not the deep shades of blue and red that are in demand for fine jewelry. As semi-precious or "fancy" stones, they have value, however.

The Yogo Gulch-Judith River region is more promising, the colors varying from light blue to quite dark blue, including some of the true "cornflower" tint so much prized in the sapphires of Ceylon. Others incline to amethystine and almost ruby shades. Some of them are "peacock blue" and some dichroic, showing a deeper tint in one direction than in another; and some of the "cornflower" gems are equal to any of the Ceylonese, which they strongly resemble,—more than they do those of Cashmere. Several thousand carats were taken out in 1895, from a preliminary washing of one hundred loads of the "earth;" of these, two hundred carats were of gem quality and yielded, when cut, sixty carats of fine stones worth from $2 to $15 a carat. All, however, are small, none having yet been obtained of more than 1½ carats in weight.

Mineralogically, the Montana sapphires possess great interest. The accompanying papers of Prof. Pirsson and Dr. Pratt present the petrological and crystallographic aspects in detail, and to these the reader is further referred.

Art. XLV.—*On the Corundum-bearing Rock from Yogo Gulch, Montana;* by L. V. Pirsson.

The corundums whose occurrence and character have been described in the foregoing paper, are found in a dike of igneous rock cutting the sedimentary beds near the entrance of Yogo Gulch. While the corundums, which are washed out as gems, occur in that portion of the dike which has been highly altered and decayed, comparatively little altered material is also obtainable, and the opportunity to study a specimen and some sections cut from it the writer owes to the kindness of Mr. W. H. Weed of the U. S. Geological Survey.

In the hand specimen the rock is of a dark gray, basic appearance and has an uneven fracture. It contains light green or white included fragments which form its most conspicuous feature, and these angular inclusions are probably pieces of limestone broken off and carried upward by the fluid rock in its ascent. They vary in size from those of microscopic dimensions to some that are a centimeter across. Many of them consist entirely of calcite, while others appear to be made up wholly of a pale green mineral which is probably a pyroxene. The largest inclusions show a reaction rim of the same green pyroxene, the rim being about one millimeter thick, while the entire center is of calcite with scattered prisms of the same green pyroxene. The rock itself shows only a few scattered tablets of mica two or three millimeters in diameter as phenocrysts, while the groundmass glitters with minute flecks of biotite, and considerable pyroxene is seen.

It is in this rock that the sapphire occurs imbedded in large, distinct, well-formed crystals as described in the previous paper. They show the corroded, etched surfaces characteristic of this occurrence and often have traces of a blackish crust upon them.

Microscopical. In thin section the rock at once shows its character as a dark, basic lamprophyre, consisting mainly of biotite and pyroxene. There is a little iron ore present, but its amount is small and much less than is usually seen in rocks of this class. The biotite is strongly pleochroic, varying between an almost colorless and a strong, clear, brown tint. It occurs in ragged masses, rarely showing crystal outline, and it contains a large amount of small apatite crystals. The pyroxene is of a pale green tint with the habit of diopside and is filled with many inclusions, now altered but probably originally of glass; in some crystals these inclusions are so abundant as to render the mineral quite spongy. The grains sometimes show crystal form but are mostly anhedral and vary in size, though the evidence is not sufficient to show two distinct generations.

These two minerals lie closely crowded together and no feldspars are seen in the rock. The interstices between them consist of a small amount of a clouded, brownish, kaolin-like aggregate, which appears to represent some former feldspathoid component, possibly leucite, perhaps analcite. The rock appears to have its closest affinities in the monchiquite group, of which it may be considered a basic, somewhat altered type. The abundance of biotite shows its relation to the minettes, but the rock is much richer in the ferro-magnesian components and lacks the feldspar of the minettes. It has evidently a close affinity with the minettes and shonkinite of the region whose occurrence has been already described,* and is clearly a more basic form of the same magma. It has the same richness in biotite and pyroxene as these, but differs in the feldspathic component. The Yogo Peak center is but a small number of miles distant from the locality.

Some calcite in agglomerated granules is also seen in the section and this, as is so often the case in lamprophyres, does not appear as if secondary in origin and is probably due to limestone fragments picked up as previously mentioned.

Origin of the sapphires. The occurrence of such well-crystallized corundum in a basic igneous rock is of great interest. It seems clear, from the many different ways in which this mineral occurs, that there must be several methods in nature for its formation. The association with metamorphic rocks such as gneisses, schists, etc., is well known and its occurrence with granites is also not uncommon. In all these cases, however, the association is with older, metamorphic or granular crystalline rocks, and we know of its occurrence in more recent, undoubted, basic, igneous rocks in but few cases. Lagorio,† in an article to be mentioned presently, gives a list of the known occurrences of corundum in igneous rocks, their tuffs, ejected fragments and contact zones. The number of occurrences where the mineral is found imbedded in igneous rocks is small, and to them the author can add Unkel on the Rhine and Steinheim near Frankfort on the Main, where, as he has observed, small blue sapphires enclosed in the fresh basalt have been found.

By a series of important and interesting experiments Morozewicz‡ showed that molten glass of a basic character dissolved alumina readily and in large quantity, and from this, on cooling, corundum and spinel crystals separated out. Lagorio,§ in commenting on these results and adding details of some experiments of his own, showed that the former idea which had been held concerning the origin of corundum in igneous rocks

* This Journal, vol. l, 1895, p. 467.
† Zeitschr. für Kryst., vol. xxiv, p. 285, 1895.
‡ Ibid., vol. xxiv, p. 281, 1895. § Op. cit., supra.

should now no longer be urged. This idea was that such corundums had been torn loose from some place below where they had previously existed, and being infusible had spread themselves through the magma. Others again recognized in these corundums infusible but recrystallized portions of rock fragments enclosed in the magma, other portions being converted into spinel, cordierite, etc. Lagorio points out, however, that this could not be the case, as corundum dissolves in molten glasses, and he calls attention to the confusion which has existed between *fusibility* of compounds in molten masses and their *solubility* in the same, the two being quite distinct. The characteristic form of corundum occurring with igneous rocks is the thin, flat, hexagonal table with low rhombohedron, described in the following paper.

This occurrence at Yogo Creek is an important addition to the list of pyrogenetic corundum. The clear-cut form of the crystals and their general distribution shows that they have crystallized out of the magma with as much certainty as the well formed phenocrysts of feldspar in a porphyry betray their origin.

The general character of the rock, however, and its close relationship to the minettes and shonkinite of the region shows that it could not originally have been sufficiently rich in alumina to have allowed a general separation out of corundum. The condition of it, as mentioned above, shows that the magma took up great quantities of inclusions from the sediments through which it passed. Among these sediments must have been a great, though unknown, thickness of the Belt formation, consisting of clay shales. This formation lies between the Archæan gneisses and the lowest beds of recognized Cambrian. The liability of the beds to be shattered by igneous rocks ascending through it and included as fragments, has already been shown elsewhere.*

Such included fragments of shale, if the magma maintained its heat sufficiently, as confined in dike form it naturally would do, would eventually be dissolved, as the experiments described show. There would thus be formed local areas in the magma very rich in alumina, which, on cooling, would allow crystals of corundum to separate out. This explanation seems to us most in accord both with the facts observed in the field and those obtained by experiment in the laboratory. The form of the crystals is also in accord with that of the pyrogenetic corundums.

This occurrence then agrees well with the experiments and views of Lagorio and is indeed an important confirmation of them.

Mineralogical Petrographical Laboratory, Sheffield Scientific School, Yale University, New Haven, June, 1897.

* Geology of Castle Mt., Bull. 139, U. S. Geol. Survey, p. 72.

Art. XLVI.—*On the Crystallography of the Montana Sapphires;* by J. H. Pratt.

The sapphire crystals from Yogo Gulch, Montana, are etched and striated to such a degree that no crystallographic measurements were possible on the reflecting goniometer; but sufficiently accurate angles could be obtained with the contact goniometer to allow of the identification of the faces.

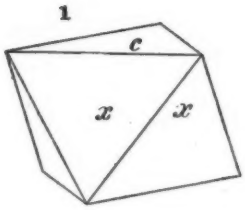
The prism of the second order $a(11\bar{2}0)$ which is so common on corundum was not observed on any of the crystals from this locality. The only two faces that could be identified were the base $c(0001)$ and the rhombohedron $x(30\bar{3}2)$ which is a new face for corundum. On one crystal, two very small faces were observed, which were too small to be measured with the contact goniometer, but were probably the faces of a pyramid of the second order.

In determining the rhombohedron, ten or more independent measurements were made of $c \wedge x$. These varied from 66° to 68°, but approximated closely to 67°, which agrees very well with the calculated value, 67° 3′, for $0001 \wedge 30\bar{3}2$.

The crystals are developed as shown in figs. 1, 2 and 3, page 425, the prevailing type being like fig. 3. The crystals vary from those where the base is very largely developed, having a diameter of 8mm, while the rhombohedron is only 1mm, to those that have the base and rhombohedron equally developed. (Fig. 1). Where the faces are more equally developed, the rhombohedral faces are generally rounded.

The basal plane often shows characteristic striations which are parallel to the three intersections of the base c, and the rhombohedron x, as shown in fig. 4. These lines are sharp and distinct and on the very flat crystals can easily be measured, when examined under the microscope. The rhombohedral faces are very roughly striated without showing any distinct parallel lines.

One very common development of these crystals is a repeated growth on the basal plane, of the rhombohedron $x(30\bar{3}2)$ and the base, $c(0001)$, as represented in fig. 2. These growths are very varied, as is shown in figs. 11–14 (p.427), where they are drawn in basal projection. In fig. 11, there is but one secondary rhombohedron and base, which has one of its rhombohedron faces a continuation of one of the rhombohedron faces of the crystal. Fig. 12 represents a repeated growth, each face of which is entirely distinct from the faces of the main crystal. In fig. 13 there are represented two and in fig. 14 a series of such growths, where a number of the rhombohedral faces coincide.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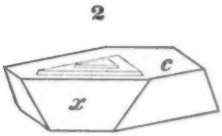
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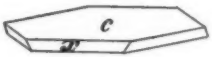
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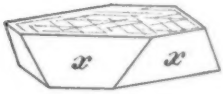
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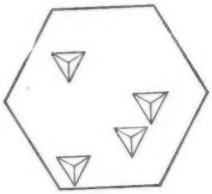
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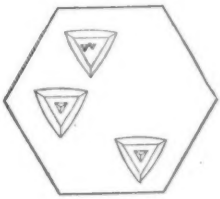
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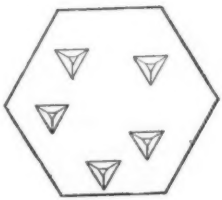
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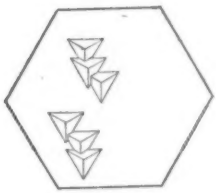
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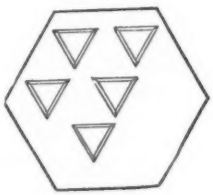
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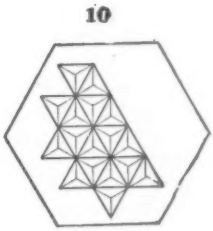

10

These growths occur most frequently on the flat crystals. The thickness of the rhombohedron rarely reaches 1^{mm} and often they are so thin, that they appear like striations. Figs. 11*a*–14*a*, representing the same crystals as figs. 11–14, have been drawn as they appear under the lens, which brings out the relation of the base and rhombohedron to better advantage.

Bauer* in a recent article, entitled "Ueber das Vorkommen der Rubine in Birma," has described this same style of development as occurring on the Burma rubies, but it is not so general as on the Montana corundums.

Etching-figures.—The etching-figures, which were observed on nearly all the crystals examined, were on the basal plane. The figures are very perfect, and although showing many different forms, they all have a rhombohedral symmetry. Fig. 5 represents the common etching-figure, which is a rhombohedral depression terminating in a point. The edges of the depression are sharp and well-defined, as are also the intersections of the rhombohedral faces of the depression. These rhombohedral faces were smooth and gave fair reflections of the signal on the reflecting goniometer. In measuring them, all the crystal but the depression to be measured was covered with a thin coating of wax. Two different crystals were measured, which gave for rhombohedron on rhombohedron 22° 30′; this corresponds to the rhombohedron $10\bar{1}7$, for which the calculated value is 21° 50′. The same style of figures were observed whose edges were parallel to those of the negative rhombohedron; these, however, are not common in isolated figures.

Another common form is represented in figs. 6 and 7, where the depression is bounded by the basal plane, which at times is so large that the rhombohedral plane is hardly visible. Fig. 8 represents etching-figures, where, on the basal plane of a shallow depression, there is another and sometimes two other etching-figures. These second etching-figures are like the common ones shown in fig. 5. The outer rhombohedral contour of these figures is generally rounded; this is also usually the case with the deeper depressions.

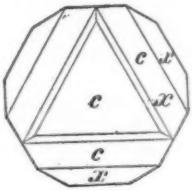
Often the etching-figures are intergrown (fig. 9) and when many of these occur together they have the appearance of raised figures, rather than of depressions. This raised appearance is very striking, when there is a combination of the plus and minus rhombohedron in parallel position and without overlapping each other (fig. 10).

The figures vary considerably in size, but most of them are near 1^{mm} in diameter. A few were observed that were nearly 2^{mm} in diameter.

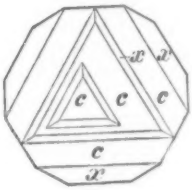
* Neues Jahrbuch für Min. Geol. und Pal., ii, p. 209, 1896.

11

12



13

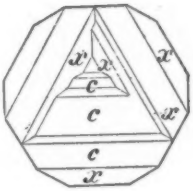


11*a*

12*a*

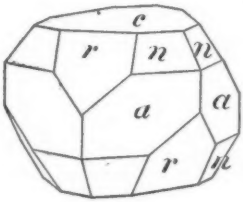
13*a*

14



14*a*

15



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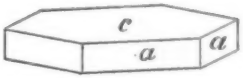

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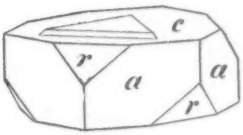


Bauer* has described etching-figures that he observed on the base 0001 and the pyramid $22\bar{4}3$ of the Burma rubies. Those on the base are similar to the figures in fig. 5, except that the outside contour of the rhombohedron is rounded.

Sapphires from Emerald Bar, Montana.

The crystals from Emerald Bar, Cañon Ferry, Meagher Co., Montana, are entirely different in their development from those from Yogo Gulch. The prism $a(11\bar{2}0)$ is always present and is usually in combination with the base $c(0001)$ and the unit rhombohedron, $r(10\bar{1}1)$, fig. 17. On some of the crystals, however, the rhombohedron is wanting and the prism is very short as represented in fig. 16. Fig. 15 represents a crystal terminated by a pyramid of the second order in addition to the base and rhombohedron. The measured angles only approximate to the calculated ones for the face $22\bar{4}3$, but as this is the common pyramid for corundum it seems very probable that it is the face. The crystal was only well terminated at one end. The crystal is similar to one figured by Bauer from the Burma district.

The crystals are all rough and more or less striated, so that the measurements with the contact goniometer were only approximate, but were sufficiently accurate to identify the faces.

The repeated growth described above was also observed on the Emerald Bar crystals but not in any variety of forms. Only one form of growth was observed, represented in fig. 17, page 427, which is a combination of the unit rhombohedron and the base.

None of the etching-figures so common on the Yogo Gulch crystals were found on these crystals.

Mineralogical Petrographical Laboratory,
Sheffield Scientific School, New Haven, Conn.

* L. c., p. 213.

Art. XLVII.—*Electrical Measurement by Alternating Currents;* by Henry A. Rowland.

The electrical quantities pertaining to an electric current which it is usually necessary to measure, outside of current, electromotive force, watts, etc., are resistances, self and mutual inductances and capacities. I propose to treat of the measurement of alternating currents, electromotive force and watts in a separate paper. Resistances are ordinarily best dealt with by continuous currents, except liquid resistances. I propose to treat in this paper, however, mainly of inductances, self and mutual, and of capacities together with their ratios and values in absolute measure as obtained by alternating currents. I also give a few methods of resistance measurement more accurate than usually given by means of telephones or electrodynamometers as usually used and specially suitable for resistances of electrolytic liquids.

I have introduced many new and some old methods, depending upon making the whole current through a given branch circuit equal to zero. These always require two adjustments and they must often be made simultaneously. However, some of them admit of the adjustments being made independently of each other, and these, of course, are the most convenient. But all these zero methods do not admit of any great accuracy unless very heavy currents are passed through the resistances. The reason of this is that an electrodynamometer cannot be made nearly as sensitive for small currents as a magnetic galvanometer. The deflection of an electro-dynamometer is as the square of the current. To make it doubly sensitive requires double the number of turns in *both* the coils. Hence we quickly reach a limit of sensitiveness. It is easy to measure an alternating current of ·0001 ampere and difficult for ·00001 ampere. A telephone is more sensitive and an instrument made by suspending a piece of soft iron at an angle of 45°, as invented by Lord Rayleigh, is also probably more sensitive.

For this reason I have introduced here many new methods, depending upon adjusting two currents to a phase-difference of 90° which I believe to be a new principle. This I do by passing one current through the fixed and the other through the suspended coil of an electrodynamometer. By this means a heavy current can be passed through the fixed coils and a minute current through the movable coil, thus multiplying the sensitiveness possibly 1000 times over the zero current method.

I have also found that many of the methods become very simple if we use mutual inductances made of wires twisted

together and wound into coils. In this way the self inductances of the coils are all practically equal and the mutual inductances of pairs of coils also equal. Hence we have only to measure the minute difference of these two to reduce the constants of the coil to one constant, and yet by proper connections we can vary the inductances in many ratios. Three wires is a good number to use. However, the electrostatic induction between the wires must be carefully allowed for or corrected if much greater accuracy than $\frac{1}{1000}$ is desired.

By these various methods the measurement of capacities and inductances has been made as easy as the measurement of resistances, while the accuracy has been vastly improved and many sources of error suggested.

Relative results are more accurate than absolute as the period of an alternating current is difficult to determine, and its wave form may depart from a true sine curve.

Let self inductances, mutual inductances, capacities and resistances be designated by L or l, M or m, C or c, and R or r with the same suffixes when they apply to the same circuit, the mutual inductance having two suffixes. Let b be 2π times the number of complete periods per second, or $b = 2\pi n$. The quantities $b\mathrm{L}$, $b\mathrm{M}$ or $\frac{1}{b\mathrm{C}}$ are of the dimensions of resistance and thus $\frac{\mathrm{L}}{\mathrm{M}}$, $b^2\mathrm{LC}$ or $b^2\mathrm{MC}$ have no dimensions. $b^2\mathrm{LM}$, $\frac{\mathrm{L}}{\mathrm{C}}$ or $\frac{\mathrm{M}}{\mathrm{C}}$ have dimensions of the square of resistances.

Where we have a mutual inductance M_{12}, we have also the two self inductances of the coils L_1 and L_2. When these coils are joined in the two possible manners, the self inductance of the whole is

$$\mathrm{L}_1+\mathrm{L}_2+2\mathrm{M}_{12} \text{ or } \mathrm{L}_1+\mathrm{L}_2-2\mathrm{M}_{12}.$$

In case of a twisted wire coil the last is very small. Likewise $\mathrm{L}_1\mathrm{L}_2-\mathrm{M}^2_{12}$ will be very small for a twisted wire coil, as is found by multiplying the first two equations together.

If there are more coils we can write similar equations. For three coils we have

$$\begin{aligned}
&\mathrm{L}_1+\mathrm{L}_2+\mathrm{L}_3+2\mathrm{M}_{12}+2\mathrm{M}_{13}+2\mathrm{M}_{23}\\
1.\quad &\mathrm{L}_1+\mathrm{L}_2+\mathrm{L}_3-2\mathrm{M}_{12}-2\mathrm{M}_{13}+2\mathrm{M}_{23}\\
2.\quad &\mathrm{L}_1+\mathrm{L}_3+\mathrm{L}_3-2\mathrm{M}_{12}+2\mathrm{M}_{13}-2\mathrm{M}_{23}\\
3.\quad &\mathrm{L}_1+\mathrm{L}_3+\mathrm{L}_3+2\mathrm{M}_{12}-2\mathrm{M}_{13}-2\mathrm{M}_{23}
\end{aligned}$$

Connecting them in pairs, we have the self inductances

$$\begin{array}{lll}
\mathrm{L}_1+\mathrm{L}_2+2\mathrm{M}_{12} & \mathrm{L}_1+\mathrm{L}_3+2\mathrm{M}_{13} & \mathrm{L}_2+\mathrm{L}_3+2\mathrm{M}_{23}\\
\mathrm{L}_1+\mathrm{L}_2-2\mathrm{M}_{12} & \mathrm{L}_1+\mathrm{L}_3-2\mathrm{M}_{13} & \mathrm{L}_2+\mathrm{L}_3-2\mathrm{M}_{23}
\end{array}$$

There are many advantages in twisting the wires of the standard inductance together, but it certainly increases the

electrostatic action between the coils. This latter source of error must be constantly in mind, however, and, for great accuracy, calculated and corrected for. But by proper choice of method we may sometimes eliminate it.

For the most accurate standards, I do not recommend the use of twisted wire coils, at least without great caution. But for many purposes it certainly is a great convenience, especially where only an accuracy of one per cent is desired. In some calculations I have made, I have obtained corrections of from one to one-tenth per cent from this cause.

For twisted wires the above results reduce to $3L+6M$, $3L-2M$. Similar equations can be obtained for a larger number of wires. For twisted wire coils, n wires joined abreast, the self induction is $\frac{L+(n-1)M}{n}$, which is practically equal to L or M. The resistance is R/n.

When we have $n=p+m$ wires twisted and wound in a coil and we connect them p direct and m reverse, the resistance and self induction will be

$$\frac{nR^3+b^2R[AC+BC-nAB]}{(nR)^2+(bC)^2} \text{ and } \frac{R^2[n(A+B)-C]+b^2ABC}{(nR)^2+(bC)^2}$$

where R is the resistance of one coil and

$$A=L+(n-1)M$$
$$B=L-M$$
$$C=nL+(4mp-n)M$$

This gives self inductances and resistances equal or less than L and R. The correction for electrostatic induction remains to be put in. For the general case, the equation is very complicated for coils abreast, with mutual inductances.

The number of mutual inductances to be obtained is M for two wires, 0, M, 2M for three wires, 0, M, 2M, 3M for four wires, etc. From these results we see that we are always able to reduce mutual to self inductance. Measuring the self inductance of a coil connected in different ways, we can always determine the mutual inductances in terms of the self inductances.

Thus we need not search for methods of directly comparing mutual inductances with each other, although I have given two of these, but we can content ourselves with measuring self inductances and capacities. Fortunately most of the methods are specially adapted to the latter, the ratio of self inductance to capacity being capable of great exactness by many methods.

In the use of condensers I have met with great difficulty from the presence of electric absorption. I have found that this can be represented by a resistance placed in the circuit of the condenser, which resistance is a function of current period

I have developed Maxwell's theory of electric absorption in this manner. Correcting his equations for a small error, I have developed the resistance and capacity of a condenser as follows:

Let a condenser be made of strata of thicknesses $a_1 a_2$, etc. and specific induction capacities $k_1 k_2$, etc. and resistances $p_1 p_2$, etc. Then we have

$$R = \frac{B_0}{b^2} - \frac{B_2}{b^4} + \frac{B_4}{b^6} - \text{etc.}$$

$$\frac{1}{C} = A_0 - \frac{A_2}{b^2} + \frac{A_4}{b^4} - \text{etc.}$$

Where

$$B_0 = \frac{a_1}{r_1 k_1^2} + \frac{a_2}{r_2 k_2^2} + \text{etc.}$$

$$B_2 = (4\pi)^2 \left\{ \frac{a_1}{r_1^3 k_1^4} + \frac{a_2}{r_2^3 k_2^4} + \text{etc.} \right\}$$

$$B_4 = (4\pi)^4 \left\{ \frac{a_1}{r_1^5 k_1^6} + \text{etc.} \right\}$$

etc.

$$A_0 = 4\pi \left\{ \frac{a_1}{k_1} + \frac{a_2}{k_2} + \text{etc.} \right\}$$

$$A_2 = (4\pi)^3 \left\{ \frac{a_1}{r_1^2 k_1^3} + \frac{a_2}{r_2^2 k_2^3} + \text{etc.} \right\}$$

etc.

Mr. Penniman has experimented in the Johns Hopkins University laboratory with condensers by method 25 and found some interesting results. With a mica standard condenser of $\frac{1}{3}$ microfarad he was not able to detect any electric absorption, although I have no doubt one of the more accurate methods will show it.

With a condenser, probably of waxed paper, he found

Number of complete periods per second.	Capacity in microfarads.	Apparent resistance in ohms.
14·0	4·64	139·6
32·0	4·96	34·1
53·3	4·96	20·5
131·1	4·94	5·2

The first value of the capacity seems to be in error, possibly one of calculation. However, the result seems to show a nearly constant capacity but a resistance increasing rapidly with decrease of period, as Maxwell's formulæ shows. The constant value of the capacity remains to be explained.

Mr. Penniman will continue the investigation with other condensers, liquid and solid, as well as plates in electrolytic liquids.

The results in the other measurements have been fairly satisfactory, but many of the better methods have only been recently discovered and are thus untried. But we must acknowledge at once that work of the nature here described is most liable to error. Every alternating current has, not only its fundamental period, but also its harmonics, so that very accurate absolute values are almost impossible to be obtained without great care. To eliminate them, I propose to use an arrangement of two parallel circuits, one containing a condenser and the other a self-inductance, each with very little resistance. The long period waves will pass through the second side and the short ones through the condenser side. By shunting off some of the current from the second side, it will be more free from harmonics than the first one.

However, in a multipolar dynamo, especially one containing iron, there is danger of long period waves also, which this method might intensify. A second arrangement, using the condenser side, might eliminate them. However, many dynamos without iron and without too many poles and properly wound produce a very good curve without harmonics, especially if the resistance in the circuit is replaced by a self inductance having no iron. These remarks apply only to absolute determinations. Ratios of inductance, self and mutual, and capacity are independent of the period, and thus it can always be eliminated. Measurements of resistances also are independent.

But there are other errors which one who has worked with continuous currents may fall into. Nearly all alternating currents generate electromagnetic waves which are so strong that currents exist in every closed circuit with any opening between conductors in the vicinity.

We eliminate this source of error by twisting wires together and other expedients. But in avoiding one error, we plunge into another. For, by twisting wires we introduce electrostatic capacity between them, which may vitiate our results. Thus, in methods 23 or 24 for comparing mutual inductances, if there is electrostatic capacity between the wires, a current will flow through the electrodynamometer in the testing circuit and destroy the balance.

Various expedients suggest themselves to eliminate this trouble, as, for instance, the variation of the resistance A in the above, but I shall reserve them for a future paper. I may say, however, that it is sometimes possible, as in method 12 for instance, to choose a method in which the error does not exist.

However, with the best of methods, much rests with the experimenter, as errors from electromagnetic and electrostatic induction are added to errors from defective insulation when we use alternating currents.

These errors are generally less than one per cent, however, and intelligent and careful work reduces them to less than this.

The following methods generally refer by number to the plate on which the resistances, etc. are generally marked. One large circle with a small one inside represent an electrodynamometer. Of course the circuit of the small coil can be interchanged with the large one. Generally we make the smaller current go through the hanging coil.

By the methods 1 to 14, we adjust the electrodynamometer to zero by making the phase difference in the two coils 90°. For greatest sensitiveness, the currents through the two coils must be the greatest possible, heating being the limit. This current should be first calculated from the impedance of the circuit, as there is danger of making it too great.

In the second series of methods, 15–26, the branch circuit in which the current is to be 0 is indicated by 0.

Resistances in the separate circuits are represented by R R′ R, etc. and r r' r, etc. Corresponding self inductances and capacities in the same circuits are L L′ L, etc. and l l' l, etc. or C C′ C, etc. and c c' c, etc. $b=2\pi n$ where n is the number of complete current waves per second.

The currents must be as heavy as possible, $\frac{1}{10}$ ampere or more, and it is well to make those that require a current of more than $\frac{1}{100}$ ampere of larger wire freely suspended in oil. A larger current can, however, be passed through an ordinary resistance box for a second or two without danger. A few fixed coarse resistances of large wire in air or oil with ordinary resistance boxes for fine adjustment, are generally all that are required. Special boxes avoiding electrostatic induction are, however, the best, but are not now generally obtainable.

In some methods, such as 8, 9, 10, etc. we can eliminate undesirable terms containing the current period by using a key which suddenly changes the connections before the period has time to change much.

In using twisted wire mutual inductances, methods 7 and 12 are about or entirely free from error due to electrostatic action between the wires. In all the methods this error is less when the resistance of the coils is least and in 23 and 24 when A is least. In method 8 the error is very small when the coil resistances and R are small and r great. In this method with 1 henry and 1 microfarad the error need not exceed 1 in 1000. Probably the same remarks apply to 9, 10, 11, also. By suitable adjustment of resistances in the other method, the error

may be reduced to a minimum. It can, of course, be calculated and corrected for.

An electrodynamometer can be made to detect ·0001 ampere without making the self inductance of the suspended coil more than ·0007 henrys or that of the stationary coils more than ·0006 henrys, the latter coil readily sustaining a current of $\frac{1}{10}$ amperes without much heating.

An error may creep in by methods 1-14 if the current through the suspension is too great, thus heating it and possibly twisting it. This should be tested by short circuiting the suspended coil or varying the current. For the zero method it is eliminated by always adjusting until there is no motion on reversing the current through one coil.

Inductances containing iron introduce harmonics and vary with current strength. Thus they have no fixed value.

Closed circuits or masses of metal near a self inductance, diminish it, and increase the apparent resistance which effects vary with the period. Short circuits in coils are thus detected.

Electrolytic cells act as capacities which, as well as the apparent resistance, vary with the current period. They also introduce harmonics. The same may be said of an electric arc.

An incandescent lamp or hot wire introduces harmonics into the circuit.

Hysteresis in an iron inductance acts as an apparent resistance in the wire almost independent of the current period, and does not, of itself, introduce harmonics. The harmonics are due to the variation of the magnetic permeability with the amount of magnetization.

Electric absorption in a condenser acts as a resistance varying with the square of the period, the capacity also varying, as I have shown above.

In general any circuit containing resistances, inductances and capacities combined acts as a resistance and inductance or capacity, both of which vary with the current period, the square of the current period alone entering. For symmetry the square of the current period can alone enter in all these cases and those above.

Hence only inductances containing no iron or not near any closed metallic circuits have a fixed value. The same may be said of condensers, as they must be free from electric absorption or electrolytic action to have constants independent of the period. There is no apparent hysteresis in condensers and the constants do not apparently vary with the electrostatic force.

The following numbers indicate both the number of the method and the figures in the plate, p. 437.

Method 1.

$$\frac{L'}{c} = \frac{[r(R_{,}+R'')+R_{,,}(r+R_{,})]\,[R'(R_{,}+R_{,,})+R''(R_{,}+R')]}{(R_{,}+R''+R_{,,})^2}$$

Method 2.

$$\frac{L'}{c} \text{ or } b^2LL' \text{ or } -\frac{1}{b^2cc'} = \frac{[R_{,,}R'-R_{,}R'']\,[R_{,}(r+R'')+R_{,,}(r+R_{,})]}{R_{,,}(R_{,}+R_{,,})}$$

Method 3.

In (1) make $R' = R'' = R_{,,} = 0$ or in (2) make $R'' = R_{,} = 0$

$$R_{,,} = \infty, \quad \frac{L'}{c} = rR'$$

In case the circuit r contains some self inductance, l, we can correct for it by the equation

$$\frac{L'}{c} = rR'\left(1 - \frac{1}{b^2lc}\right)$$

Method 4.

$$\frac{L_{,}}{c} = \frac{[R'(r+R_{,,})+R''(R'+R_{,,})]\,[R'(R''+R_{,,})+R''(R_{,}+R_{,,})]}{R'R''}$$

Method 5.

$$\frac{L_{,}}{c} = \frac{[R_{,}(R''+R_{,,})+R_{,,}(R''+R')]\,[R'(R''+R_{,,})+r(R'+R'')]}{(R'+R'')\,(R''+R_{,,})}$$

Method 6.

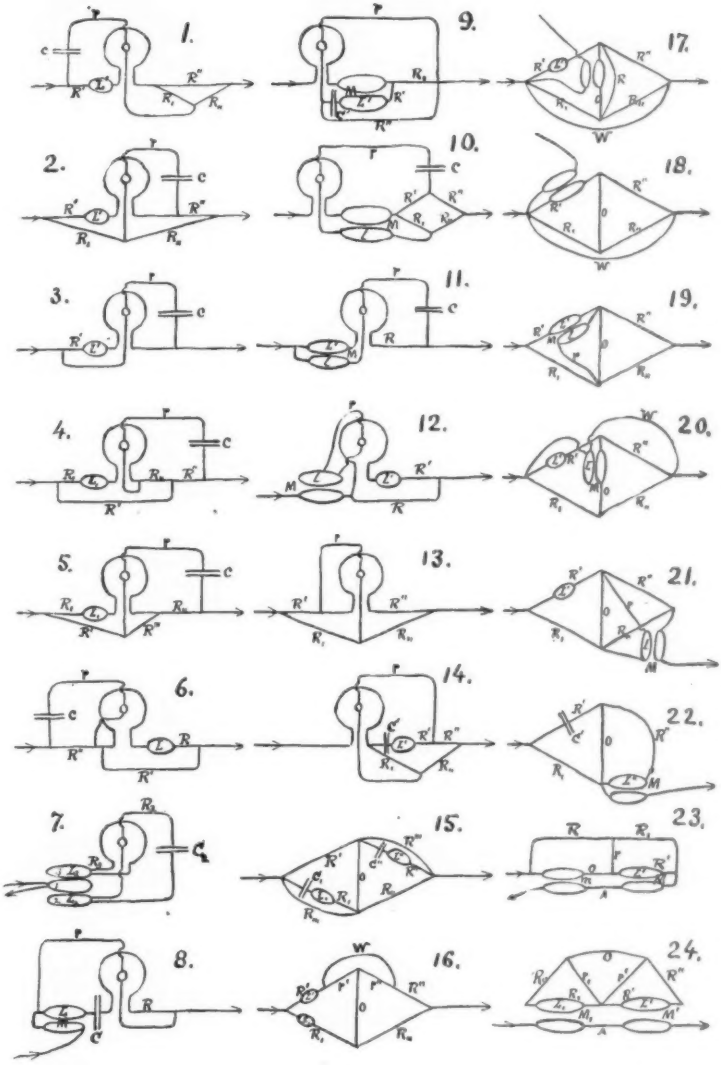
$$\frac{L}{c} \text{ or } \frac{l}{C} = (R+R')\,(R''+r)$$

We can correct for self inductions, L', L'' in the circuits R', R'' by using the exact equation

$$b^2\left[[L'(r+R'')+\left(L''-\frac{1}{b^2c}\right)R'\right]\left[L''(R+R')+R''(L+L')\right]+R'R''(r+R'')\,(R+R')$$

or approximately

$$\frac{L}{c} = (R+R')\,(R''+r) - \frac{L'}{c} - \frac{L''}{c}\frac{R+R'}{R''} + b^2\frac{L[L'(r+R'')+L''R']}{R'} + \text{etc.}$$



In methods 1 to 14 inclusive the concentric circles are the coils of the electrodynamometer. Either one is the fixed coil and the other the hanging coil. Oblong figures are inductances and when near each other, are mutual inductances. A pair of cross lines is a condenser.

Method 7.

$$R_3R_3M_{12}M_{13}+b^2[L_3M_{12}-M_{23}M_{13}]\,[L_2M_{13}-M_{23}M_{13}]=0$$

For a coil containing three twisted wires, $M_{12}=M_{13}=M_2$, and the self inductions of the coils are also equal to each other and nearly equal to the mutual inductions. Put an extra self induction L_3 in R_3 and a capacity C_2 in R_2. Replace L_3 by $L+L_3$ and L_2 by $L-\frac{1}{b^2C_2}$ and we can write

$$\frac{L_3+L-M}{C_2}=R_2R_3+b^2(L-M)\,(L_3+L-M).$$

As $L-M$ is very small and can be readily known, the formula will give $\frac{L_3}{C_2}$. When $L-M=0$ we have

$$\frac{L_2}{C_3}\text{ or }\frac{L_3}{C_2}=R_2R_3$$

Method 8.

$$b^2M(M+L)=rR \qquad 2b^2M^2=rR+(rR)'$$
$$\text{or } b^2M(M-L)=(rR)' \qquad 2b^2LM=rR-(rR)'$$

Placing a capacity in the circuit R, we have also

$$b^2M(M+L)-\frac{M}{C}=rR$$

$$\text{or } b^2M(M-L)+\frac{M}{C}=rR$$

In case the coil is wound with two or more twisted wires, $M-L$ is small and known. For two wires, $M-L$ is negative. For three wires, two in series against the third, M can be made nearly equal to 2L. Hence M, L and C can be determined absolutely, or C in terms of M or vice versa.

To correct for the self induction, l, of r we have the exact equations

$$b^2M(M+L)=rR+b^2l(L+M)$$
$$b^2M(M-L)=rR+b^2l(L-M)$$
$$b^2M(M+L)-\frac{M}{C}=rR-b^2l\left(L+M-\frac{1}{b^2C}\right)$$
$$b^2M(M-L)+\frac{M}{C}=rR-b^2l\left(L-M-\frac{1}{b^2C}\right)$$

If the condenser is put in r, we have

$$\frac{L+M}{c}=rR-b^2M(L+M)$$

$$\text{or } \frac{L-M}{c}=rR+b^2M(L-M)$$

Method 9.

$$b^2\mathrm{L'M} - \frac{\mathrm{M}}{\mathrm{C'}} = \mathrm{R}_t\left[\mathrm{R'} + \mathrm{R}_t + \frac{r\mathrm{R''}}{r+\mathrm{R''}}\right]$$

$$\text{or } -b^2\mathrm{L'M} + \frac{\mathrm{M}}{\mathrm{C'}} = \mathrm{R}_t\left[\mathrm{R'} + \mathrm{R}_t + \frac{r\mathrm{R''}}{r+\mathrm{R''}}\right]$$

Making $\mathrm{R''} = \infty$ and $r + \mathrm{R'} = r$ we have

$$-b^2\mathrm{L'M} + \frac{\mathrm{M}}{c'} \text{ or } b^2\mathrm{L'M} - \frac{\mathrm{M}}{\mathrm{C'}} = \mathrm{R}_t(r + \mathrm{R}_t)$$

Taking two observations we can eliminate $b^2\mathrm{L'M}$ and we have

$$\frac{\mathrm{M}}{\mathrm{C'}} = \mathrm{R}_t\{r - (r)'\}$$

Knowing L'M we can find C'. Throwing out C' (i. e., making it ∞) we can find $b^2\mathrm{L'M}$ in absolute measure: then put in C' and find its value as above.

To correct for self induction in R_t, we have for case $\mathrm{R''} = \infty$, the exact equation

$$b^2\mathrm{L'M} - \frac{\mathrm{M}}{\mathrm{C'}} = \mathrm{R}_t(r + \mathrm{R}_t) + b^2[\mathrm{L'} + \mathrm{L}_t - \mathrm{M}]\mathrm{L}_t - \frac{\mathrm{L}_t}{\mathrm{C'}}$$

The correction, therefore, nearly vanishes for two twisted wires in a coil where $\mathrm{L'} - \mathrm{M} = 0$ and C is taken out.

Method 10.

$$-b^2l\mathrm{M} + \frac{\mathrm{M}}{c} \text{ or } b^2l\mathrm{M} - \frac{\mathrm{M}}{c} =$$

$$\frac{[\mathrm{R}_t\mathrm{R''} - \mathrm{R}_{tt}\mathrm{R'}]\{r[\mathrm{R'} + \mathrm{R''} + \mathrm{R}_t + \mathrm{R}_{tt}] + (\mathrm{R'} + \mathrm{R}_t)(\mathrm{R''} + \mathrm{R}_{tt})\}}{[\mathrm{R'} + \mathrm{R''} + \mathrm{R}_t + \mathrm{R}_{tt}]^2}$$

This can be used in the same manner as 9 to which it readily reduces. But it is more general and always gives zero deflection when adjusted, however M is connected. To throw out C make it ∞. .

Method 11.

$$\frac{\mathrm{L} - \mathrm{M}}{c} = r\mathrm{R} + b^2(l - \mathrm{M})\ (\mathrm{L} - \mathrm{M})$$

$$\frac{\mathrm{L} + \mathrm{M}}{c} = r\mathrm{R} + b^2(l + \mathrm{M})\ (\mathrm{L} + \mathrm{M})$$

For the upper equation the last term may be made small and the method may be useful for determining $\mathrm{L} - \mathrm{M}$ when c is known. Method 8, however, is better for this.

Method 12.

$$\frac{L'}{l} = \frac{R+R'}{r}$$

Should the circuits R and r also have small self inductances, L and l, we can use the exact equation

$$L'+L = \frac{R+R'}{r}\, l\, \frac{1+\dfrac{Lr}{lR}}{1-\dfrac{b^2Ll}{rR}} = \frac{R+R'}{r}\, l\left[1+\frac{Lr}{lR}+\frac{b^2Ll}{rR}+\text{etc.}\right]$$

When L′ and l are approximately known, we can write the following, using the approximate value on the right side of the equation

$$\frac{L'}{l} = \frac{R+R'}{r}\left[1+\frac{Lr}{lR}-\frac{L'}{l}\,\frac{r}{R+R'}+\frac{b^2Ll}{rR}+\text{etc.}\right.$$

Taking out L′ and putting a condenser, C, in R we have

$$\frac{l}{C} = rR' - b^2lCR(R+R')$$

For a condenser, R can be small or zero.

Method 13.

$$\text{(A)}\quad \left[bL''-\frac{1}{bC''}\right]^2 = \frac{[R_{,,}R'-R_{,}R'']\,[R_{,,}(r+R_{,})+R_{,}(r+R'')]}{R_{,}^{\,2}}$$

This determines capacities or self inductions in absolute value. As described above, mutual induction can also be determined by converting it into self induction.

$$\text{(B)}\quad \left[bL_{,,}-\frac{1}{bC_{,,}}\right]^2 = \frac{[R''R_{,}-R'R_{,,}]\,[R_{,,}(r+R_{,})+R_{,}(r+R'')]}{R'(r+R_{,})}$$

$$\text{(C)}\quad \left[bL_{,}-\frac{1}{bC_{,}}\right]^2 = \frac{[R'R_{,,}-R''R_{,}][R_{,}(r+R'')+R_{,,}(r+R_{,})]}{R''[r+R''+R_{,,}]}$$

Method 14.

$$bL'-\frac{1}{bC'}\Big]^2 =$$

$$\frac{[R_{,}R''-R_{,,}R']\,[r[R'+R_{,}+R''+R_{,,}]+[R'+R_{,}]\,[R''+R_{,,}]\,]}{R_{,,}[r+R''+R_{,,}]}$$

Of course, in any of these equations, methods 13 or 14, L″ is eliminated by making L″ = 0 or the condenser, C, is omitted by making C = ∞.

Method 15.

$$-\frac{1}{b^2C_iC''} \text{ or } b^2L_iL'' \text{ or } -\frac{L_i}{C''} =$$

$$\frac{R'R_{ii}(R_i+R_{iii})(R''+R''')-R''R_iR'''R_{iii}}{R'R_{ii}-R'''R_{iii}}$$

$$\frac{C''}{C_i} \text{ or } \frac{L_i}{L''} \text{ or } -b^2L_iC'' = \frac{R_{iii}R'''R_i-R'R_{ii}(R_i+R_{iii})}{R'R_{ii}(R''+R''')-R''R_{iii}R'''}$$

When $R_{iii}=\infty$ we have

$$\frac{L_i}{C''}=\frac{R'R_{ii}(R''+R''')-R''R_iR'''}{R'''}=R'R_{ii}-\frac{R''}{R'''}[R'''R_i-R'R_{ii}]$$

$$b^2L_iC''=\frac{R'''R_i-R'R_{ii}}{R''R'''}$$

If we adjust by continuous current, we shall have $R'''R_i - R'R_{ii}=0$. For a condenser we can make $R''=0$ provided there is no electric absorption. In this case b^2L_iC'' is indeterminate and we can adjust to find $\frac{L_i}{C''}$. However, two simultaneous adjustments are required.

But I have shown that the presence of electric absorption in a condenser causes the same effect as a resistance in its circuit, the resistance, however, varying with the period of the current. Hence R'' must include this resistance. However, the value of R'' will not affect the first adjustment much and so the method is easy to work. If it is sensitive enough it will be useful in measuring the electric absorption of condensers in terms of resistance.

It has the advantage of being practically independent of the current period for $\frac{L}{C}$ as it should be.

For comparison of capacities the same simplification does not occur.

Indeed the method is of very little value in this case, being surpassed by 16.

Method 16.

$$(A)\ [R_iR''-R_{ii}R'][W+r'+r'']+W[R_ir''-r'R_{ii}]=0$$

$$\frac{L'}{L_i} \text{ or } \frac{C_i}{C'}=\frac{R''}{R_{ii}}+\frac{(Wr'')}{R_{ii}(W+r'+r'')}$$

The first equation is satisfied by adjusting the Wheatstone bridge so as to make

$$(R_iR''-R_{iii}R')=0 \quad R_ir''-R_{ii}r'=0 \quad R_i(R_{ii}+r'')-R_{ii}(R'+r')=0$$

That is

$$\frac{R_{,}}{R_{,,}} = \frac{R'}{R''} = \frac{r'}{r''}$$

We can then adjust W with alternating currents. This is a very good method and easy of application but requires many resistances of known ratio. Many of these, however, may be equal without disadvantage. A well known case is given by making r' and $r''' = 0$.

(B) By placing self inductions or condensers in $R_{,}$ and r'' instead of the above we have the following

$$\frac{c''}{C'} \text{ or } -b^2 L_{,} c'' \text{ or } \frac{L_{,}}{l''} = \frac{R_{,}(W+R'')-R'R_{,,}}{R''(W+r'+r'')Wr''}$$

$$-\frac{1}{b^2 C_{,} c''} \text{ or } \frac{L_{,}}{c''} \text{ or } -b^2 L_{,} l'' = \frac{(W''+r'+r'')(R_{,}R''-R_{,,}R')+W(R_{,}r''-R_{,,}r'')}{W+R''}$$

Making $R'' = 0$ we have

$$\frac{c''}{C_{,}} \text{ or } -b^2 L_{,} c'' \text{ or } \frac{L_{,}}{l''} = \frac{R_{,}W-R'R_{,,}}{Wr''}$$

$$-\frac{1}{b^2 C_{,} c''} \text{ or } \frac{L_{,}}{c''} \text{ or } -b^2 L_{,} l'' = R'R_{,,} + r'R_{,,}\left(1+\frac{R'}{W}\right) - \frac{r''}{W}(R_{,}W - R'R_{,,})$$

In case we adjust the bridge to $R_{,}W - R'R_{,,} = 0$ and a condenser is in r'' so that we can make $r'' = 0$, the value of $-b^2 L_{,} c''$ will be indeterminate and we can find $\frac{L_{,}}{c''}$ by the adjustment of W alone.

This is an excellent method, apparently, as only one adjustment is required.

However, see the remarks on method 15. This present method $r''=0$ for $\frac{L}{c}$ is Anderson's with, however, alternating currents instead of direct as in his.

The other two values are imaginary in this case. Indeed the whole method, B, is only of special value for $\frac{L}{c}$, as two adjustments are needed for the others.

Method 17.

(A) $W = \infty$. $R = \infty$

$$b^2 M L' = R_{,}R'' - R_{,,}R'$$

$$\frac{L'}{M} = \frac{R'+R_{,}+R''+R_{,,}}{R_{,,}}$$

By this method the self induction of the mutual induction coil is eliminated. But it is difficult to apply, as two resistances must be adjusted and the adjustment will only hold while the current period remains constant. The same remarks apply to B and C following.

(B) $R = \infty$

$$b^2ML' = \frac{W[R_iR'' - R'R_{ii}]}{W''' + R_i + R_{ii}}$$

$$\frac{L'}{M} = \frac{W'''[R' + R_i + R'' + R_{ii}] + (R' + R_i)(R'' + R'')}{R_{ii}W}$$

(C) $W = \infty$

$$b^2ML' = \frac{R}{R + R'' t R_{ii}}(R_iR'' - R'R_{ii})$$

$$\frac{L'}{M} = \frac{R(R' + R_i + R'' + R_{ii}) + (R' + R_i)(R'' + R_{ii})}{RR_{ii}}$$

Method 18.

$$R_iR'' - R'R_{ii} = 0$$

$$\frac{L'}{M'} = 1 + \frac{R''}{R''} + \frac{R' + R''}{W}$$

L′ and M′ belong to the same coil. By adjusting the Wheatstone bridge first, W can then be afterwards adjusted.

To find the ratio for any other coil independent of the induction coil, we can first find $\frac{L'}{M'}$ as above, Then add L to the same circuit and we can find $\frac{L + L'}{M'}$. Whence we can get L. This seems a convenient method if it is sensitive enough, as the value of $\frac{L'}{M'}$ should be accurately known for the inductance standard.

Method 19.

$$b^2(L'l - M^2) = \frac{r}{R_{ii}}[R'R_{ii} - R''R_i]$$

$$\frac{L'}{M} = \frac{R' + R_i}{r} - b^2\frac{L'l - M^2}{r^2}\left(\frac{l}{M} + 1\right) = \frac{R' + R_i}{r} - \frac{R'R_{ii} - R''R_i}{rR_{ii}}\left(\frac{l}{M} + 1\right)$$

This is useful in obtaining the constants of an induction standard. For twisted wires $L'l - M^2$ should be nearly 0, depending, as it does, on the magnetic leakage between the coils. $\frac{l}{M}$ is often known sufficiently nearly for substitution in the right hand member. It can, however, be found by reversing the inductance standard.

Method 20.

$$R'R_{,,} - R''R_{,} = 0$$

$$\frac{M}{L} = \frac{R''}{R_{,} + R_{,,}}; \quad \frac{M}{L'} = \frac{W}{R''} \frac{R_{,,}^{2}}{(R_{,} + R_{,,})^{2}}; \quad \frac{L}{L'} = \frac{W}{R' + R''} \quad L > M; \quad L' \text{ any value.}$$

In case of a standard inductance, M and L are known, especially when the wires are twisted.

The method can then be used for determining any other inductance, L′, and is very convenient for the purpose.

$R_{,,}$ and $R_{,} + R_{,,}$ are first calculated from the inductance standard. The Wheatstone bridge is then adjusted and W varied until a balance is obtained. This balance is independent of the current period, as also in the next two methods.

Method 21.

$$R'R_{,,} - R''R_{,} = 0$$

$$\frac{l}{M} = \frac{R' + R_{,}}{R_{,}}; \quad \frac{L'}{M} = \frac{(R' + R_{,})^{2}}{rR_{,}}; \quad \frac{L'}{l} = \frac{R' + R_{,}}{r} \quad l > M.$$

This is Niven's method adapted to alternating currents. See remarks to method 20.

Methods 20 and 21 are specially useful when one wishes to set up an apparatus for measuring self induction, as the resistances R′, R″, $R_{,}$, $R_{,,}$ can be adjusted once for all in case of a given inductance standard and only W or r need be varied afterwards.

Method 22.

$$\frac{L''}{M} = \frac{R' + R_{,}}{R_{,}}; \quad \frac{M}{C'} = R_{,}R''; \quad \frac{L''}{C'} = R''(R' + R_{,})$$

This is Carey Foster's method adapted to alternating currents and changed by making R″ finite instead of zero.

The ratio of $R' + R_{,}$ to $R_{,}$ is computed from the known value of the induction standard. R″ is then adjusted and C′ obtained. In general the adjustment can be obtained by changing $R_{,}$ and R″. The adjustment is independent of the current period.

Method 23.

$$b^{2}mL' = rR_{,} + R[r + R' + R_{,}]$$

$$\frac{Mr - L'R}{m} = r + R' + R_{,}$$

If we make R = 0 we have

$$b^2 m L' = r R_{,}$$

$$\frac{M}{m} = \frac{r + R' + R_{,}}{r}$$

This method requires two simultaneous adjustments. M must also be greater than m. As M and L′ belong to the same coil, we can consider this method as one for determining m in terms the M and L′ of some standard coil.

The resistance, A, can be varied to test for, or even correct, the error due to electrostatic action between the wires of the induction standard.

Method 24.

$$\frac{L_{,}}{M_{,}} = \frac{L'}{M'}\,\frac{r_{,}}{r'};\quad \frac{M_{,}}{M'} = \frac{r'(r_{,} + R_{,} + R_{,,})}{r_{,}(r' + R' + R'')}$$

This is a good method for comparing standards. We first determine $\frac{L_{,,}}{M}$ for each coil by one of the previous methods. Then we can calculate $\frac{r_{,}}{r'}$ and adjust the other resistances to balance.

It is independent of the period of the current and suitable for standards of equal as well as of different values, as the mutual inductances can have any ratio to each other.

For twisted wire coils $r_{,} = r'$ very nearly. See method 23 for the use of the resistance, A.

Method 25.

In fig. 6 remove the shunt R′ and self induction L.

This method then depends upon the measurement of the angular deflection when a self induction or a capacity is put in the circuit of the small coil of the electrodynamometer and comparing this with the deflection, when the circuit only contains resistance.

The resistance of the circuit, r, is supposed to be so great compared with R that the current in the main circuit remains practically unaltered during the change.

There is also an error due to the mutual induction of the electrodynamometer coils which vanishes when r is great.

$$\frac{1}{b^2 c^2} \text{ or } b^2 l^2 = R''[r + R'']\left[\frac{r_{,} + R_{,}''}{R_{,}''}\,\frac{\theta_{,}}{\theta} - \frac{r + R''}{R''}\right]$$

These formulas assume that the deflection is proportional to

θ. This assumption can be obviated by adjusting $\theta = \theta'$ when we have

$$\frac{1}{b^2c^2} \text{ or } b^2l^2 = \frac{(r+R'')(r_{,}R''-rR_{,}'')}{R}$$

These can be further simplified by making $R'' = R_{,}''$.

The method thus becomes very easy to apply and capable of considerable accuracy. As the absolute determination depends on the current period, however, no great accuracy can be expected for absolute values except where this period is known and constant, a condition almost impossible to be obtained. The comparison of condensers or of inductances is, however, independent of the period and can be carried out, however variable the period, by means of a key to make the change instantaneously.

Method 26.

Similar results can be obtained by putting the condenser or inductance in R'' instead of r, but the current through the electrodynamometer suspension is usually too great in this case unless r is enormous. We have in this case for equal deflections,

$$\frac{1}{b^2C''^2} \text{ or } b^2L''^2 = R''(R''+r)\left(\frac{rR_{,}''-r_{,}R''}{r_{,}R''}\right)$$

Where $r_{,}$ and $R_{,}''$ are the resistances without condenser or self induction.

This is a very good method in many respects.

For using 25 and 26, a key to make instantaneous change of connections is almost necessary.

To measure resistance by alternating currents, a Wheatstone bridge is often used with a telephone.

I propose to increase the sensitiveness of the method by using my method of passing a strong current through the fixed coils of an electrodynamometer while the weaker testing current goes through the suspended system.

Using non-inductive resistances, methods 10, 13 A, B, C, and 14 all reduce to proper ones. 10 or 14 is specially good and I have no doubt will be of great value for liquid resistances. The liquid resistances must, however, be properly designed to avoid polarization errors. The increase of accuracy over using the electrodynamometer in the usual manner is of the order of magnitude of 1000 times.

Since writing the above I have tried some of the methods, especially 6 and 12, with much satisfaction. By the method

12, results to 1 in 1000 can be obtained. Replacing L′ by an equal coil, the ratio of the two, all other errors being eliminated, can be obtained to 1 in 10,000, or even more accurately.

The main error to be guarded against in method 12, or any other where large inductances or resistances are included, arises from twisting the wires leading to these. The electrostatic action of the leads, or the twisted wire coils of an ordinary resistance box, may cause errors of several per cent. Using short small wire leads far apart, the error becomes very small.

Method 6 is also very accurate, but the electric absorption of the condensers makes much accuracy impossible unless a series of experiments is made to determine the apparent resistance due to this cause.

In method 12 I have not yet detected any error due to twisting the wires of coils *l*. However, the electrostatic action of twisted wire coils is immense and the warning against their use which I have given above has been well substantiated by experiment. Only in case of low resistances and low inductances or in cases like that just mentioned is it to be tolerated for a moment. Connecting two twisted wires in a coil in series with a resistance between them, I have almost neutralized the self inductance, which was one henry for each coil or four henrys for them in series!

Altogether the results of experiment justify me in claiming that these methods will take a prominent place in electrical measurement especially where fluid resistances, inductances and capacities are to be measured. They also seem to me to settle the question as to standard inductances or capacities, as inductances have a real constant which can now be compared to 1 in 10,000, at least.

The new method of measuring liquid resistances with alternating currents allows a tube of quite pure water a meter long and 6^{mm} diameter having a resistance of 10,000,000 ohms to be determined to 1 in 1,000 or even 1 in 10,000. The current passing through the water is very small, being at least 500 times less than that required when the bridge is used in the ordinary way. Hence polarization scarcely enters at all.

It is to be noted that all the methods 15 to 24 can be modified by passing the main current through one coil of the electrodynamometer and the branch current through the other. The deflection will then be zero for a more complicated relation than the ones given. If, however, one adjustment is known and made, the method gives the other equation.

Thus method 18 requires $R_{,} R'' - R' R_{,,} = 0$. Hence, when this is satisfied we must have the other condition alone to be

satisfied. Also in method 22, when we know the ratio of the self and mutual inductances in the coil, the resistances can be adjusted to satisfy one equation while the experiment will give the other and hence the capacity in terms of the inductances.

Again, pass a current whose phase can be varied through one coil of the electrodynamometer, and the circuit to be tested through the other. Vary the adjustments of resistances until the deflection is zero, however the phase of current through the first coil may be varied.

The best methods to apply the first modification to are 15 A, 16 A and B, 18, 20, 21, 22 and 24. In these, either a Wheatstone bridge can be adjusted or the ratio of the self and mutual inductances in a given coil can be assumed as known and the resistances adjusted thereby.

The value of this addition is in the increased accuracy and sensitiveness of the method, an increase of more than one hundred fold being assured.

As a standard I recommend two or three coils laid together with their inductances determined and not a condenser, even an air condenser.

ART. XLVIII.—*The alleged Jurassic of Texas. A Reply to Professor Jules Marcou;* by ROBT. T. HILL.

APROPOS of personal criticisms and questions of fact concerning the validity of the work of myself and others upon the later Mesozoic formations in the Southwestern United States, made by Professor Jules Marcou* in many recent publications, such as the American Geologist,† Proceedings of the Boston Society of Natural History‡, Science,§ and especially the paper entitled "Jura of Arkansas, Kansas, Oklahoma, New Mexico and Texas," in this Journal for September, 1897 (pp. 197–212), I beg to submit the following statements.

In the month of September, 1853, Professor Jules Marcou, while accompanying a rapidly marching military expedition for the preliminary determination of a Route for a Pacific Railway Survey which was traveling up the valley of the Canadian River, through Oklahoma, the Panhandle of Texas, and Northeastern New Mexico, saw two small, outlying beds of the Lower Cretaceous formation.

The first of these localities, which he termed that of Comet Creek, then in Indian Territory, is in what is now known as G County, Oklahoma, west of the present town of Arapahoe. It has recently been revisited by Mr. T. Wayland Vaughan of the United States Geological Survey and described in this Journal for July, 1897. At this spot, Professor Marcou, according to his own statement, remained "only one hour."‖

The second locality was a detached outlier of the Llano Estacado, standing in the broad valley through which the Canadian winds its way through northeastern New Mexico. Here he remained "only three or four hours." Each of these

* In my writings I have always shown the greatest respect for Professor Marcou, and still have for him the most charitable and friendly feelings. Furthermore, I have always given and shall continue to give him the fullest credit whenever credit is due. The injustice of his attacks upon me and the incorrectness of his statements, which, if unanswered, would prove serious defacements of the scientific record, force me to take note of his accusations, and to add a line of controversy to geologic literature. Professor Marcou's attacks upon the validity of my work have been so direct, numerous and skillfully introduced into the geologic literature of the day under the guise of alleged scientific discussion, that I would deem it unjust not only to myself but to my co-laborers and the United States Geological Survey, with which organization I am connected, and the scientific world in general, not to correct some of his assertions It is also at the earnest solicitation of several of my co-laborers, who have read this manuscript, that it is submitted to the public.

† Growth of Knowledge concerning the Texas Cretaceous, August, 1894.

‡ The Jura of Texas, October, 1896, pp. 149–158.

§ Science, Oct. 22, 1897.

‖ "I was enabled on account of the rapidity of the march of my military escort, to remain at Comet Creek only one hour, and at Pyramid Mount only three or four hours."—This Journal, September, 1897, p. 198.

occurrences, from two to five feet thick at one place and less than fifty at the other, represents an outlying isolated, attenuated outcrop of the great body of Lower Cretaceous which has a development of over 2,000 feet in Texas, and from which they have been disconnected by prehistoric and recent denudation, and to which I have devoted thousands of miles of travel and careful study and field work during my lifetime.

At the first of these localities he collected from a "limestone five feet thick,"* one species of Ostreid ("*Gryphæa pitcheri*") and at the second from a bed given by him as 30 feet thick, two other species of the fossil Ostreidæ (called by him *G. dilatata* and *O. marshii*). These species, with two or three hundred molluscan forms, since reported by others, are now known to constitute the faunas of the Lower Cretaceous formations of Kansas, New Mexico, and Texas.

Upon the supposed resemblance of the fossil oyster from the first mentioned of these localities to certain forms in the Cretaceous of Switzerland, and the fact that it occurred as a shell agglomerate or "lumachelle" resembling in its lithologic facies similar "lumachelle" of Switzerland, he referred the beds containing it to the "Neocomian" epoch, and has since used this determination as a basis of his subsequent discussion of this system, as other workers discovered and delineated the great series of strata and its areal extent to which this single outcrop has proved to belong. Likewise on the supposed resemblance of the two fossil oysters from Pyramid Mount in New Mexico to forms from the "Oxfordian" and "Lower Oolite groups"† of England and France, he referred these beds to the Jurassic period—an opinion which he has since rigidly maintained and used as the basis for asserting the Jurassic age of various other and entirely distinct strata since discovered and described by later writers throughout the Texas region, and coloring vast areas of what we now know to be Lower Cretaceous and Tertiary, upon maps which he has compiled.‡

By his own statement, Professor Marcou has spent not over five hours of his life-time in observation of the formations under controversy. In fact he has never seen the main body of the Cretaceous in Texas or Indian Territory at all, and has never visited the localities, nor examined the vast collections subsequently reported. "I have explored only a very small part of Texas," he says, "only a simple road in the Panhandle,"§ and I might add, that his road lay entirely through the Permian of

* U. S. Pacific Railroad Explorations, 1853–54, vol. iv, p. 43, H. Doc. 129, Washington, 1855.

† Geology of North America, Zurich, 1858, p. 19, and in several other publications.

‡ "Geology of North America." Geological Map of the World, etc.

§ This Journal, September, 1897, p. 208.

the Canadian Valley and Tertiary of the Llano Estacado, which he called Triassic and Jurassic respectively, and nowhere touched upon the Cretaceous in the State of Texas. Professor Marcou by his writings has at several times conveyed the impression that he had seen the Cretaceous in Texas.* The various journals, itineraries and maps of the Pacific Railway Expedition as published by himself and others, giving a minute record of the progress of the party day by day, show that it nowhere encountered this locality or any other south of the Ouachita Mountains. The fossils from Fort Washita and the Cross Timbers of Texas described by him in his Geology of North America, were collected and sent to him by Dr. G. G. Shumard.

Each of his localities have since been thoroughly studied by specially equipped expeditions of the United States Geological Survey and the Texas State Geological Survey. The Tucumcarri region has been twice visited by me and the results of my observations published in Science† and in this Journal‡ and elsewhere.§‖ The Texas Geological Survey also made researches in this locality, and published extensively thereon.¶ Professor Alpheus Hyatt several years ago spent a season of minute field work upon the region, and his manuscript report thereon is in the office of this Survey. Thus we have, as opposed to the three hours spent by Professor Marcou, the observations of three independent parties, who have devoted days and months to the locality. Each of these parties (although both Professor Hyatt** and myself†† were at first predisposed towards Professor Marcou's conclusions, and made the mistake in print of partially supporting him) have all arrived, after careful and impartial study, at conclusions contrary to his. I *have shown beyond all doubt that the deposits which he called Jurassic are Cretaceous—not only Cretaceous, but of a Cretaceous horizon which I believe to be of the same general formation but of a horizon stratigraphically above the rocks which he, himself, collected at Comet Creek and called "Neocomian."* It is also extremely doubtful if the Comet Creek beds are homotaxially equivalent to the Neocomian, as he alleges.

* "I have seen and studied the strata of the Upper Greensand and Marly Chalk, in the bed of Little River," etc, "and also on the Elm Fork of Trinity river"—Professor Marcou in American Geologist, August, 1894, p. 100.

† July 14, 1893.

‡ September, 1895, p. 234.

§ Report on Underground Waters, Washington, 1892.

‖ Bull. Geol. Soc. Amer., May, 1894, p. 332.

¶ Third Annual Report, pp. 201 et seq.—A controversial article fully answered in Science, July 14, 1893.

** 11th Annual Report U. S. Geological Survey, Part 1, pp. 97–100.

†† Circular letter, Austin, Texas, 1888.

Professor Marcou established his Jurassic at Tucumcarri solely upon two species of fossil oyster. Each of these expeditions, in addition to the two fossil oysters found by Professor Marcou, collected large Cretaceous faunas of over a dozen species, similar to that which occurs in Grayson County, Texas, *above* the Comet Creek horizon (Preston Beds) which he has himself referred to the Cretaceous, and published lists thereof in the publications above mentioned.* His repeated assertion that his adversaries will not or have not published figures of these fossils is unjust. These fossils are mostly all well known species which have been fully illustrated by their authors and are in the United States National Museum, the State Capitol of Texas, and at Johns Hopkins University, where they are accessible to all interested, and have been, are being, or will be duly published at the proper time and place as the systematic work of publication of the Cretaceous stratigraphy and paleontology of Texas progresses.

Professor Marcou has said† that his "observations, instead of being accepted and used for further development of our knowledge of the Texas Cretaceous, were, on the contrary, opposed systematically." This statement is true except so far as the last words are concerned, for the opposition was largely based upon independent investigation of parties who, in some instances like the writer, were predisposed to accept his conclusions. Not only has Professor Marcou rigidly maintained the fundamental errors of his conclusions as to age, but has used them as a protext for assaulting the observations, often with crimination and misquotation, of every later worker who has since more thoroughly studied the field, or distorted their language into confirmations of his own erroneous conclusions.

The foregoing is a brief statement of the facts which gave rise to a controversy, which has pervaded geologic literature for nearly forty years, and which is marked by unparalleled bitterness and accusations of American geologists as a body on the part of Professor Marcou. This controversy will be again referred to in the later pages of this paper, after we say a few words in direct reply to his article in the September number of this Journal.

A most serious, but less important defect of his paper is the fact that he omits reference to most of the recent literature which shows the fallacies of his conclusion concerning Comet Creek and Tucumcarri, and these omissions leave the general reader, who may judge the work of others by his article, under a false impression concerning the questions involved. Sys-

* Lists of these fossils were published by me in Science, Sept 1, 1895, and this Journal. Sept. 1895.

† American Geologist, August, 1894, p. 100.

tematic observations on the stratigraphy of these formations, written by me, and, at my request, by Messrs. Stanton and Vaughan, can be found in this Journal from 1877 to the present year, in the papers to be enumerated presently. Within the past few years I have paid special attention to these isolated but related localities in Kansas, New Mexico and Trans Pecos, Texas, and their relations to the Central Texas region where the main area of the Cretaceous lies in continuous section. The three papers specially showing the identity of Professor Marcou's Jurassic of New Mexico with the beds of the Washita Division in Texas, are entitled "Outlying Areas of the Comanche Series in Kansas, Oklahoma and New Mexico," with *ex parte* paleontologic determinations by T. W. Stanton and F. H. Knowlton, published in this Journal of September, 1895; "Section of the Cretaceous at El Paso, Texas," by T. W. Stanton and T. Wayland Vaughan, this Journal, vol. i, p. 21, 1896; and Additional Notes on the Outlying Areas of the Comanche Series in Oklahoma and Kansas" by T. Wayland Vaughan, this Journal, July, 1897. These three papers cover every well known essential point concerning these regions and should be read by all who wish to know the true merits of Professor Marcou's determinations of the localities discussed.

Furthermore, two previous bulletins of the Geological Society of America,* written by me upon the paleontologic and stratigraphic relations of the Cretaceous formations of Indian Territory and Texas adjacent to Red River—the localities from which the Cretaceous fossils described by Professor Marcou, in his Geology of North America, were collected—set forth the details of the comprehensive section of the Cretaceous developed in that region by which the stratigraphic position of Professor Marcou's isolated outcrops can be located in the general section. Finally, concerning the paleontology of the entirely distinct Trinity Division as published in my Arkansas Report—the only one of my papers to which Professor Marcou refers,—I will state that the paleontologic descriptions and figures of that volume were fully revised and republished by me in a paper entitled "The Invertebrate Paleontology of the Trinity Division," published in the Proceedings of the Biological Society of Washington, vol. viii, pp. 9–40, Plates I–VIII, June 3, 1893, and that this later paper, not the Arkansas Report, represents my views of the fauna discussed. In this later paper the description of the form from Arkansas described by me under the name of *Ammonites walcotti*, is fully revised by Professor Hyatt and redescribed by me, and my previous generic and specific comparisons as quoted by Marcou (p. 199)† are abandoned and superseded.

* Vol. ii, pp. 503–528, 1891, and vol. v, pp. 297–338, 1894.

† This Journal, Sept., 1897.

Professor Marcou completely ignores this paper in his writings, and seems to present me to the public as having said things which were never intended.

Not only does he fail to set forth fairly the work of others, but he even appropriates from them their own substance and converts them to his own end. No better illustration of this can be found than the sentence on page 211 where he speaks of the "true Washita Division as I established it as long ago as 1853 when at Comet Creek near Fort Washita." This assertion that he established the Washita Division is absolutely untrue. The two feet of beds at Comet Creek which he saw in 1853 (and this locality is not near Fort Washita, as he states) were never called by him the "Washita Division" or aught else but "Neocomian," nor was the term ever used in scientific literature by him until after it had been invented by another. The classification of the beds of the Texas Cretaceous into "divisions" and the term "Washita Division" was originally made by me and published in this Journal for April, 1896, and amplified in my later papers. Marcou's "Neocomian," Comet Creek *bed*, is only a single horizon in one of the eight great formations composing the Washita Division, seven of which are shown in my paper entitled the "Geology of Parts of Texas, Indian Territory, and Arkansas Adjacent to Red River (Bull. Geol. Soc. of America, March, 1894) and one of which, the Grayson Marls, has been added by Cragin.

His article is also so full of conclusions with which few Americans will agree, that we can only point out at present a few of the scientific points of disagreement. He dismisses (p. 204) Mr. Knowlton's careful study and identifications of the Dicotyledons* with the assertion that "no conclusion can be drawn from such a meagre florula." Mr. Knowlton's florula enumerates five characteristic Dakota species. The occurrence of a Dakota-like dicotyledonous flora in Marcou's "Jurassic" beds both in Kansas and also at Gallisteo, New Mexico, as has been noted by Newberry,† is certainly against his hypothesis, but paleo-botanists and geologists in general, who are acquainted with the western United States, know that this flora is distinctly Cretaceous in its facies, and not Jurassic. Professor Marcou neglects to state, and I myself had overlooked the fact, that Newberry‡ many years ago noted in New Mexico the occurrence of dicotyledonous plants with Marcou's Jurassic oyster in sandstone called Jurassic by Marcou, near Gallisteo, New Mexico. The *cycad* which the latter mentions (p. 204) (*Cycadeoidea munita* of Cragin) was found in the

* Given in my paper in this Journal of September, 1895, p. 212.
† This Journal, vol. xxviii, 1859, p. 33. ‡ Ibid.

Tertiary Plains drift, and derived from a geological position unknown, and has been studied by Professor Ward. Even if it should prove of Purbeckian age it would still be from a much higher horizon than Professor Marcou's alleged "Oxfordian," "Oolitic," Jurassic of New Mexico.

On page 198 he accuses me "of making a clean sweep of the marine American Jura," and quotes a paragraph from me in which I stated that "there are reasons for suspecting that no marine Jurassic formations of Atlantic sedimentation have as yet been discovered north of Argentina on the present Atlantic slope of the American hemisphere." In quoting this paragraph Professor Marcou apparently forgets that he, himself, has distinctly said in italics (Geology of North America, p. 19), that "*the Jurassic rocks do not exist on the Atlantic slope of North America nor anywhere east of the Mississippi River.*" My assertion of practically the same proposition is maintained by every known fact, unless the Wealden beds, which are not positively known to be marine and which are classified with the Cretaceous by a preponderance of authority, are Jurassic as maintained by Marsh—(merely a question of classification, as I have recently shown in Science*). Furthermore, as he referred the Tucumcarri beds under discussion to the "Oxfordian" and "Lower Oolite"† of the Jura, they are in no manner to be confused with the Wealden, or "Jurassic" of Marsh. Neither does the sentence quoted from me make "a clean sweep of the American marine Jura," for it in no manner alleges that there is no Jurassic on the Pacific slope, or in the Black Hills of Dakota, where, as is well known, Jurassic formations, in no manner related to those of New Mexico, so-called by Professor Marcou, do occur.

Concerning his general classification and tabular view of the whole country south of the Arkansas, pp. 208–211, I will state that it has no value and cannot in any manner be fitted to known conditions. For instance, I have shown that the Cheyenne Sandstone of Kansas which he places at the base of his section, contains dicotyledonous flora and occurs stratigraphically about midway in the Lower Cretaceous of the Texas section, and does not belong to my Trinity Division at all, as at first supposed by Cragin. His "Tucumcarri Division (B) and "Neocomian Division," are synchronous formations, and embrace beds far more nearly allied to the Gault than Neocomian. His descriptions of these divisions are purely imaginary creations, stratigraphically incorrect, and altogether out of harmony with the natural occurrence of the rocks or the literature thereon. The whole table is ingeniously constructed by compilation of the works of the very authors he condemns.

* December 18, 1896, pp. 918–922. † Geology of North America, pp. 19–20.

Professor Marcou has long imagined that brief observations in these outlying areas have constituted him an authority on the greater Texas region which he has not seen, and made them the basis for creating, at his study at Cambridge, such tabulations as above mentioned and dictating where work should and should not be done in the Cretaceous region of Texas. This article under discussion is especially profuse in such suggestions. I can dismiss them in a lump as follows: The monograph on the fossils which have been called "*Gryphæa pitcheri*," was written and ready for the printer a year ago, and was transmitted to the Director for publication on January 13, 1897. When it does appear it will further show by the most conclusive stratigraphic and paleontologic data, together with a careful study of the development of the forms, the identity of his alleged Jurassic species from Tucumcarri with the forms called "*Gryphæa pitcheri*" in the Cretaceous of Texas, and the form which he names *Gryphæa kansana* (page 203) from Kansas. For the two or three specimens of the last mentioned form which he states that he possesses, and which I have seen in his studio at Cambridge, we possess hundreds of specimens showing every stage of the development. Futhermore Mr. T. W. Stanton is now solely engaged upon the descriptive paleo-zoology of the Cretaceous of Texas, a work which I had to abandon owing to pressure of other duties, and his work will be reliable and authoritative. Professor Lester F. Ward is likewise studying in a similar manner the paleo-botany.

Prof. Marcou's remark about "special need of investigation in the vicinity of Austin and Fredericksburg" can be fully answered by stating that in addition to my previously published papers on this region, there is in type for the Eighteenth Annual Report of the U. S. Geological Survey a large and comprehensive work upon this region by Mr. Vaughan and myself, giving every detail of its stratigraphy with maps and illustrations. Furthermore, we have in process of publication, four atlas sheets of this region. I have also personally conducted Mr. Stanton over this country, and he is illustrating and describing the paleontology as fast as accurate methods will permit. He has just returned from that region, where he has made additional collections.

We have already shown that his charges that his opponents will not visit his localities are unjust. Just one year ago when I lay at Muskogee, Indian Territory, upon what was then supposed my death-bed, I turned over my camp equipment to Professor Lester F. Ward and Mr. T. Wayland Vaughan, and requested them to visit the Kansas localities, which they did. Mr. Vaughan also thoroughly studied all the outlying areas to

the south thereof, including Professor Marcou's Comet Creek locality, and his results have been fully published in this Journal for July, 1897. Professor Ward, who accompanied Mr. Vaughan to the Kansas localities, is now again in the Kansas region,* studying the fossil flora. Professor C. S. Prosser has also lately published an excellent paper on the stratigraphy of the Kansas† localities, which confirms the conclusions of Professor Marcou's opponents. The invertebrate paleontology of the same region is in process of publication by Professor Cragin.

After all the complaints pervading Professor Marcou's papers against others who are working as hard and consistently as they can upon the problems of the region, for not publishing hastily, and charging that "poor stratigraphy and poor paleontology have long enough prevailed," etc., etc., he apparently sees no inconsistency in immediately creating in this article a new species of oyster (*Gryphæa kansana*) without one word of description or illustration.

His accusation (page 201) that Dr. Charles A. White changed "the generic and specific name" of his (Marcou's) *G. pitcheri*, alias *G. rœmeri*, etc., etc., to *Exogyra forniculata* is untrue. Marcou, himself, was the first to use the generic name *Exogyra* for this species, in his first paper in the original Whipple report and elsewhere,‡ and continued to use it for some time, as shown in the extracts from his writings given on a later page. Although I believe Dr. White's *Exogyra forniculata* may be identical with Marcou's *G. pitcheri*, there is still room for much doubt upon this subject.

Inasmuch as this Gryphæa and the thickness of the beds containing it is made the basis of many charges against others by Professor Marcou, it may be well to introduce here the following extract from his own writings concerning it which will be referred to later in this paper.

His record of the thickness of the Comet Creek beds and the various names which he gave to the fossil found there ("*Gryphæa pitcheri*") and which almost exclusively composes the rock, is as follows:

1855. "This limestone is only five feet thick; it is of a whitish grey color containing an immense quantity of Ostracea which I consider (provisionally) as the *Exogyra ponderosa* Roemer; having the closest analogy with the Exogyra of the neocomian of the environs of Neufchatel."—U. S. Pacific Railroad Explorations, 1853–54, vol. iv, p. 43, H. Doc. 129, Washington, 1855.

* Prof. Ward has returned since this was written, bringing with him over forty boxes of Cretaceous fossil plants.

† Report of Kansas State Geological Survey, pp. 96–181, 1897.

‡ Geology of North America, p. 17.

1858. "This limestone is only five feet thick; it is of a whitish-gray color, containing an immense quantity of fossil Ostracea, which I consider as identical with the *Exogyra* (*Gryphæa*) *pitcheri* Mort., having the closest analogy with *Exogyra couloni*, of the Neocomian of the environ of Neufchatel (Switzerland).—Geology of North America, Zurich, 1858, p. 17.

This passage purports (Geology of North America, p. 7) to be a "*verbatim*" copy of the preceding paragraph, and is copied from the chapter in the latter work (p. 9) entitled "Extract from Report of Explorations for a Railway Route, near the Thirty-fifth Parallel of Latitude from the Mississippi River to the Pacific Ocean, etc., Washington, 1855, H. Doc. 129." It should be noted that he here, as in the preceding quotation, refers the genus to *Exogyra*.

1858. In the literal copy and translation of Professor Jules Marcou's field notes by W. P. Blake, p. 131, vol. iii of the Pacific Railway Reports, quarto edition of 1856, the Comet Creek locality near Camp 31 is described as composed of "three or four broken beds with crinoids* disseminated here and there as if the ruins were formed of a lumachelle limestone of Neocomian age. This lumachelle is formed by the fragments of *Ostrea aquila* or *couloni* or a variety, for it is smaller . . . . the four beds of lumachelle are two feet."

Concerning these notes, however, Mr. Marcou later said: "I here declare that I know nothing of the publication of the edition in quarto of these reports, and that I decline all responsibility as to the use that may have been or may hereafter be made by others of my official note books," etc. (Geology of North America, etc., Zurich, 1858, p. 1.) Nevertheless he himself, now (1897) cites them as authoritative in his recent article, p. 205.

1858. On page 27 of the Geology of North America, Mr. Marcou says, in discussing his Neocomian in America, of which this is the only locality recorded as seen by him, that "its thickness varies from 6 to 50 feet."

1862. "I have never seen Morton's original specimen. . . . . I am led to believe that I did not meet with the true *G. pitcheri* of Morton in my explorations with Captain Whipple's party. Mr. Ferdinand Roemer having the opportunity of seeing, in the company of the late Dr. Morton himself, the original specimen at Philadelphia, I naturally followed his identification of *G. pitcheri;* and if Roemer has made a mistake I was misled by his description . . . Thus we shall have three species of *Gryphæa*: 1, the *G. tucumcarrii* of the Jurassic rocks of Pyramid Mount (New Mexico); 2, the false *G. pitcheri* of Roemer and Marcou, or the false *G. pitcheri var. navia* of Conrad and Hall of the Cretaceous rocks of the false Washita River (Texas) which may be called *G. rœmeri* in honor of its first discoverer, Mr. F. Roemer, and, 3, the true *G. pitcheri* Morton, which I have never seen, and, consequently,

* This word does not occur in the French version of the notes, in which *G. couloni* is also followed by a question mark.

on which I cannot give any information as to its stratigraphical position and association with other fossils.—Proc. Boston Soc. Nat. Hist., vol. viii, p. 95, 1862."

1889. "As to the *Gryphæa pitcheri* which Mr. Hall calls *var navia* it is the true *G. pitcheri* of Morton and Roemer found by me at Comet Creek near the false Washita river.—American Geologist, September, 1889, p. 163."

1896. "The first strata of this Cretaceous system contain at Comet Creek, Fort Washita, etc., an immense number of *Gryphæa rœmeri* Marcou (formerly called *G. pitcheri* by Roemer and Marcou). The *Gryphæa arcuata* are so numerous as to recall the 'Limestone of the Lias of England, France, and Germany.' These first beds, which may be called the 'Caprina and Gryphæa Rœmeri limestone; are the bottom beds of the American Neocomian or Lower Cretaceous."—The Jura of Texas by Jules Marcou, Proc. of the Boston Soc. Nat. Hist., vol. xxvii, p. 157, Boston, October, 1896.

The foregoing extracts show that he has successively called this Comet Creek species "*Exogyra ponderosa* Roemer," "*Exogyra pitcheri*" with analogy with "*Exogyra couloni*"; "*Ostrea aquila* or *couloni?*" "*Gryphæa pitcheri*," "*Gryphæa rœmeri*," "*Gryphæa pitcheri*" and *Gryphæa rœmeri.*"

The paragraphs in Professor Marcou's paper to which I personally take exception are such as that on page 199, in which he makes direct charges upon my veracity and the motives and correctness of my work, citing "an example of carelessness, not to use a stronger word, in quoting a plain paleontological fact," which "shows how unreliable Mr. Hill is when he writes on paleontology," and accusing me of endeavoring by "extraordinary alteration" and misquotation of D'Orbigny to make a certain species of Ammonite appear of Cretaceous instead of Jurassic affinities.

These accusations on Professor Marcou's part are absolutely without foundation, as anyone can see by comparing my original assertion with his extraordinary misrepresentation of it. What I said was as follows:*

Careful examination of the literature and specimens of the Boston and Washington libraries and museums failed to reveal any figured species with which this one can be identified. *It resembles* generically the group Harpoceratidæ (genus Ludwigia, Boyle), which is peculiar to the upper jurassic of Europe and also *Ammonites yo, D'Orb*, of the lower neocomian. The absence of this ammonite from the great mass of the Trinity strata, except in the place indicated, suggests that it may be an older fossil reimbedded in the Trinity, but its preservation and delicacy of structure would seem to render this impossible.

* Neozoic Geology of Arkansas, p. 128.

By omitting all the words of this paragraph except those in italics, which he brings together and by substituting the word "Cretaceous" for "Neocomian," he succeeds in establishing his remarkable construction of a proposition which I did not utter, upon which he could base his assertion that I "undertook to change the age of *Ammonites yo*," which cannot be explained otherwise than that I "wanted to sustain my classification of the Trinity Division in the Cretaceous, quoting in his (my) favor the great D'Orbigny." These charges are repudiated by every passage referring to the species of Ammonites to be found elsewhere in the volume referred to, in which I repeatedly present the Jurassic affinities of this form; but as he well knows, in another paper, the species was revised by me and the previous description in the work which he quoted was abandoned. All the other passages referring to the age of *Ammonites walcotti*, both in the Arkansas Report,* and the revision thereof are here reproduced in full, and I beg the candid reader to compare them with Professor Marcou's statement, in order to see if there is ground for his accusations.

Arkansas Report, p. 125.—Reviewing the stratigraphic evidence afforded by Trinity formation, it seems to be clearly older than any Cretaceous rocks hitherto described in this country, a fact which is verified by the paleontology as shown in the next chapter.

The stratigraphic position beneath the lowest Comanche series, which is of very early cretaceous (neocomian), and the extreme difference in the character of the sediments and fossils, confirm the opinion that the rocks are either uppermost jurassic, lowest cretaceous (Wealden) or transitional jura-cretacic. They are at least older than the oldest American cretaceous rocks hitherto known, and mark the littoral stages which characterized the beginning of the first grand subsidence of cretaceous times.

Proceedings of the Biological Society,† pp. 37–38: Only one specimen of this species has thus far been discovered. It occurred in association with *O. franklini*, *Vycaria lujani*, *Eriphyla arkansaensis*, and other mollusks herein described. The form very much resembles in outward appearance the figures of the genus *Oxynoticeras* of Hyatt, as given by Zittel and Steinman in their Manuals, but Professor Hyatt refers it to *Neumayria*, and contributes the following comments upon the specimen:

"Your *Ammonites walcotti* is probably a *Neumayria*. The aspect is Jurassic, but this group, Upper Jura, and the species

* Neozoic Geology of Southwestern Arkansas. By Robert T. Hill, Assistant Geologist.—Annual Report of the Geological Survey of Arkansas for 1888, vol. ii, pp. 125–128.

† Paleontology of the Cretaceous Formations of Texas. The Invertebrate Paleontology of the Trinity Division, by Robert T. Hill.—Proceedings of the Biological Society of Washington, vol. viii, pp. 37–38, June 3, 1893.

nearest *walcotti* occurs in the very top of the Jura of Central Volga stage, supposed by some to be similar to the Purbeck in the upturn at Malm. The obscuration of a portion of the sutures occurs over the most important part of the outer side, and the structure of the abdomen, which is rounded and has no keel, is not very consistent with the reference either to the *Neumayria* of the Jura or the so-called *Neumayria* of the Cretaceous. Nevertheless it agrees better with those of the Jura than the Cretaceous ones referred to the same genus by Nikitin."

Whatever may be the range of this genus in Europe, the writer is inclined to the belief, from the stratigraphy and association, that its occurrence in Arkansas is lowest Cretaceous, and Professor Hyatt's opinion serves to strengthen the position of the writer in his reticence in earlier papers in expressing a more definite assignment of the Trinity beds before minutely studying the accompanying faunas. The specimen was collected in the banks of Town Creek, one mile southeast of Murfreesboro, Arkansas. Named in honor of Mr. C. D. Walcott.

Nowhere in these writings do I quote or have I quoted D'Orbigny, and even my citation of a doubtful resemblance to a species of his (which citation was entirely abandoned in the revision of the species), cannot be interpreted as a quotation. The very first sentence of the paragraph upon which Professor Marcou constructs this charge distinctly shows that no identity between the species was intended. Whether Mr. Marcou's assertion that D'Orbigny's species came from the Jurassic and not the Cretaceous is true or not, I do not know (for no copy of D'Orbigny's *Paleontologie Française* is accessible to me to verify his references), but even if it is true, the matter is entirely secondary to the entire tenor of my writings and was set right by myself through its omission in the later publications.

Professor Marcou states on the same page that "he has shown with accuracy and details in the American Geologist, Dec., 1889, . . . that the whole fauna without a single exception is composed of Jurassic fossils." I am perfectly aware of the fact that in his paper cited,* he took the list of fossils illustrated by me, species for species, and asserted† their identity or resemblance, according to his fancy, with some Jurassic species of Europe, making them allied to forms from various horizons of Europe, such as the "Portlandian," the "French Jura," "Argovian," "Sequanian," the "Upper Lias," and the "Kimmeridian." These mere assertions are all the "accuracy and detail given." His identifications have so little basis of fact that I merely pass them by unnoticed and do not yet

* Jura Neocomian and Chalk of Arkansas; by Jules Marcou, American Geologist, December, 1889.

† Ibid., pp. 362–363.

accept them. This fauna is undoubtedly one of the oldest of the Comanche Series. In my Arkansas report I said that it resembled the Wealden and Purbeckian, a position which I still maintain, and Professor Marcou has proved nothing further concerning it. He has issued similar manifestoes upon the appearance of other lists of species from the Southwest, notably the one identified by Stanton and published by Dumble from the Washita Division at Kent.* Here he takes a dozen or more of the best known and commonest fossils, not of Wealden affinities, but from the uppermost division of the Lower Cretaceous, and refers each of them serially to Jurassic forms. His *ipse dixit* is all the basis there is for such correlations.

The following extracts from Professor Marcou's discussions of a species of Ammonites, a family of much more value for stratigraphic correlation than Ostreidæ, show that he, rather than others, used the peculiar methods in paleontological discussions which he has attributed to them. He found no Ammonites in the "Jurassic" Tucumcarri region, and noted their supposed absence. In his "Geology of North America" (p. 33, Plate I, fig. 1), he gave an excellent figure of a "*Cretaceous*" species which he named *Ammonites shumardii*, after Dr. George G. Shumard, here called by him "the learned geologist of Arkansas," who collected all the species from Fort Washita and Texas, near Red River. These localities, which Marcou has never seen, are several hundred miles distant from Tucumcarri, and judging from his writings he is ignorant of their stratigraphy, although they have been visited several times by the writer and made a special study by him.†

Furthermore, as I have seen, this species of Ammonite occurs by the hundreds in the Red River localities from which Marcou's type specimen was sent, in a horizon stratigraphically below beds containing the majority of the species now known to constitute the fauna of his alleged "Jurassic" of the Tucumcarri region, and *above* the horizon of his Comet Creek "Neocomian."

In 1888 Professor Alpheus Hyatt found this confessedly Cretaceous species of Professor Marcou's in the supposed "Jurassic" beds of the Tucumcarri region of New Mexico. Professor Marcou, since the latter event, endeavored to reconcile these facts in a most remarkable manner. Without awaiting publication by Professor Hyatt, and upon what authority we do not know—for Professor Hyatt has never published other than a brief administrative report‡ on his work so far as

* American Geologist, November, 1893.

† See papers previously cited.

‡ Eleventh Annual Report U. S. Geological Survey, Part 1, pp. 97–100.

I am aware — Professor Marcou immediately proceeded to announce, that* "The fauna of the upper part of the Jurassic strata of Pyramid Mount at the Tucumcarri, thanks to the collection made there in 1889 by Prof. A. Hyatt, is now well known."

Later, however, when he pressed Professor Hyatt, who has, perhaps wisely, kept out of the controversy, for an opinion concerning the age of the Ammonite, he received a very decisive answer as follows:† "I think there can be no reasonable doubt that it belongs to the Inflatus group of the genus *Schlœnbachia*, hitherto found only in the Cretaceous."

Not daunted by this decisive contradiction of the Jurassic age of this species by Professor Hyatt, Professor Marcou next proceeded to force the species into his Jurassic system, whether or no, by making a new paleontologic law to suit the case as follows: "When considered in connection with the surrounding fauna of the Tucumcarri area, the Schlœnbachia found there indicates that in America the genus appeared near the end of the Jurassic epoch, a fact constantly indicated for many other fossil forms which appeared sooner in America than in Europe."‡

To demonstrate the last proposition he asserts that he§ (Marcou), "received a very remarkable confirmation" of "his" opinion concerning "the appearance of the Jurassic genus *Schlœnbachia* during the Jurassic epoch in America," and says that Aguilera gives a description, with figure, of a Schlœnbachia "found among a whole Jurassic fauna" at Catorce. By consulting the work referred to‖ it is seen that no such statement is made, and that the Mexican Schlœnbachia is not reported with the other Jurassic fossils described by Aguilera, but is the only fossil found in the limestone of the upper part of the upper division of their section, and is referred by him to the upper part of the Lower Cretaceous.¶

The fact that he, himself, had originally given the Ammonite a Cretaceous position—an insurmountable obstacle to its alleged Jurassic occurrence in New Mexico—was further remedied as follows: Mr. Dumble found a specimen of *Ammonites leonensis* at Kent** in the same bed with the cognate of *G. pitcheri*, which Marcou confesses†† is his alleged

* American Geologist, August, 1894, p. 102.

† "The Jura of Texas," by Jules Marcou,—Proc. Bost. Soc. Nat. Hist., vol. xxvii, p. 155.

‡ Same publication as above, p. 155.

§ Proc Bost. Soc. Nat. Hist., p. 155.

‖ Boletin de la Commission de Mexico, Num. 1, pp. 49 and 50 (Mexico, 1895).

¶ Ibid., p. 49. ** Previously cited, p. 462.†† "The Jura of Texas," p. 153.

Jurassic *Gryphæa dilatata* of the Tucumcarri region. To offset this additional evidence of the Cretaceous age of the beds, Mr. Marcou, after showing to his own satisfaction that all the common species collected by Mr. Dumble from these uppermost beds of the Comanche Series are "Jurassic," erroneously says that the "*Ammonites leonensis* Conrad is not that species at all, but the *Ammonites shumardii* Marcou. "It is true that I placed this species in the Cretaceous of Texas, but I was impressed by its form. . . . If the specimen had come to me with a *Gryphæa tucumcarri*, I should not have hesitated to refer it to the Jurassic of Texas. But it came to my hands collected by a person not a geologist, who put together all the fossils obtained during a military march through Texas." It is needless to say that the two species of Ammonites cited are quite distinct. Such is an example of how he, himself, has by misquotation changed the geologic age of an Ammonite and thus gained support for his erroneous conclusions—an act identical with that which he has so skillfully tried to fix on me in the case of *A. walcotti*.

Another unfounded accusation is his assertion that I have misquoted him when I spoke of the Comet Creek bed "as being composed of a single bed of limestone five feet thick," which he alleges is "another example of want of exactness in quotation in Mr. Hill" (p. 205). The alleged quotation on my part is from the first two of the extracts from his writings previously given, wherein he distinctly says "This limestone" (not limestones) "is five feet thick." On the other hand, the only passage of Marcou's writings seen by me in which this formation is spoken of as more than one bed, is in Blake's publication of his (Marcou's) notes—the same which he has hitherto repudiated with such vehemence but which he now, for the first time in forty years, cites as authority as already noted. But in doing this he even misquotes these notes, which distinctly say that there are three or four of these beds, not five as Professor Marcou now alleges they state (p. 204). It is likewise apparent that Marcou in his various writings has himself variously given a thickness of "2, 5" and from "to 50" feet to these beds. The truth of the matter, as shown by Mr. Vaughan in his recent paper,* is that they are probably only two feet thick.

Such are some of the examples of Professor Marcou's perverted charges wherein he states that I, who, according to his own statement, am the only man in American science who has endeavored to treat him with courtesy and give his writings due credit, and who has tried to record facts truthfully, have misquoted and corrupted paleontologic facts with a motive.

* This Journal, July, 1897.

After perusing the foregoing pages one cannot but wonder why Professor Marcou should wrongfully accuse others of maliciously misquoting. Let the reader put side by side the two first abstracts we have given from his writings concerning the Comet Creek *Gryphæa* on pp. 17–18. The second of these was published by him as a verbatim copy* of the first, yet in this alleged verbatim copy he has changed the name of every species mentioned in the original and made other additions, and this, too, without one word of explanation. These instances together with the misquotations elsewhere given of Aguilera the Mexican Geologist, of himself and myself, are but a few of the many examples which could be given showing that he has certainly exceeded the ordinary limits of toleration in such practices, in which I have never, intentionally, indulged in, as he charges.

In the paper in the September number of this Journal, Professor Marcou also accuses Messrs. Hall, Roemer, Shumard, Gabb, Charles A. White, Hill, Cragin and Stanton of confusing species, and I can but consider it an honor that he should have selected my head, above all these distinguished authorities, upon which to pour the last and most concentrated dregs of his wrath. His many papers are bristling with similar assaults devoted to denouncing the scientific value of the work of James D. Dana, James Hall, J. S. Newberry, F. B. Meek, W. P. Blake, T. A. Conrad, J. D. Whitney, C. A. White, J. J. Stevenson, with side notes on nearly every American geologist of the past fifty years, against whom as a whole he has also launched certain epithets. It can be readily seen that his assaults upon me are felt less keenly when one considers the distinguished company with which I have been placed by him. This will be made still more apparent by the following brief résumé of the controversy which he has so long conducted.

The invalidity of Professor Marcou's conclusions concerning the Jurassic age of the New Mexican locality was early shown in many papers by the principal American geologists, of the decade of 1855–1865, among whom were W. P. Blake, B. F. Shumard, J. S. Newberry, F. B. Meek, T. A. Conrad, James Hall, James D. Dana, Lesquereux and others. The whole substance of the controversy and proof of the inaccuracy of Professor Marcou's conclusions, have been ably set forth by Professor Dana in this Journal for November, 1858, p. 323, and January, 1859, pp. 137–141. This was the original Marcou controversy, which died out in the year 1867. The later and detailed studies in the field by the present school of geologists, have confirmed by stratigraphic research that these

* See Geology of North America, p. 7.

older writers were correct in their affirmation of the Cretaceous age of the alleged Jurassic beds of New Mexico.

From 1867 to 1884 there was a cessation in the flow of publication from Professor Marcou's fertile pen, which did not resume until after the appearance of the writer's first papers on the geology of the Texas region, in 1886, after I had endeavored to give a résumé of Marcou's work in Oklahoma and New Mexico.* In attempting to give him credit, however, I apparently started Professor Marcou's pen again—which he resumed, after seventeen years of silence during which his history was a blank to me. Since this time his contributions have been as frequent and pointed as before. Time does not permit me to enumerate or refer to all of Professor Marcou's publications. They are all marked by similar statements to those given in the article which has brought forth this paper, only differing in the violence of the personalities indulged in.

His publications have been particularly severe in their denunciation of all American geologists. Professor James D. Dana, who has always been considered as the embodiment of honor and integrity, is accused of "distorting and misrepresenting facts,"†‡ of falsifying titles of his (Marcou's) papers,"§ of "persecuting‖ and waging war upon him," of having "filled up his Journal, since he is geological editor, with papers of controversial nature, without a single observation made in the field or museums" and charged with "persistent and blind resistance against progress," "opposition *a outrance* and his *parti pris* to ignore a system." He also states that Dana and Hall have not excuses of distance to travel over or want of facilities and opportunities to create their colossal error." "His (Dana's) efforts during 44 years have been directed to keeping life in wrong conclusions and in the opposite direction of the truth," and together with James Hall "has misled those who followed their views by various paleontological determinations and false classification."¶ He accuses Professor F. B. Meek—the ideal of exactness in paleontologic method—of "mixing strata together without regard to stratigraphy, lithology, or even paleontology," and states that Professor J. J. Stevenson makes use of language "such as it is impossible even to quote it."**

His assaults upon American geologists reached their climax, however, in his paper on "American Geological Classification

* Bull. 52, U. S. Geological Survey.
† "A Reply to the Criticisms of James D. Dana," by Jules Marcou, Zurich, 1859.
‡ Geology of North America, Zurich, 1858, p. 7.
§ American Geological Classification, by Jules Marcou, Cambridge, 1888, p. 9.
‖ Ibid., pp. 22, 23.
¶ Ibid., p. 39.
** American Geologist, Sept., 1889, p. 156.

and Nomenclature," published at Cambridge, 1888. This is a bitter attack upon nearly every American geologist of the past half century, all of whom except myself, of whom Mr. Marcou was then fulsome in his praise (for reasons elsewhere explained), are accused of outrageous personal conduct, such as "suppressing facts," "falsifying," "misquoting," "incompetent observations," etc., etc., and speaks of "the constant and utmost opposition" of Messrs. James Hall, T. Sterry Hunt, W. E. Logan, James D. Dana, the two Professors Rogers, and Professor C. H. Hitchcock, and to this list he adds in his own handwriting on page 8 of the copy sent to me, the name of Charles D. Walcott. Also, on page 11, he accuses Professor Dana "in accordance with his usual practice of giving credit to those to whom it does not belong, and pretending that the Lower Silurian is called Champlain by Mather." On page 17 he accuses Mr. Walcott of having been "misled by the erroneous notions constantly and perversely put forward by Mr. Dana."

I could quote from his various writings many other such denunciations, chiefly directed at Professor Dana as the head and leader of American geologists, just as he now assails me because I have been a pioneer in the late studies of the Mesozoic in the Texas region. I could fill a volume with similar attacks upon other men of science, such as his accusations of a like kind against Newberry, Hall, Stevenson and others. It was owing to the error of his deductions, his habit of absorbing to his own credit every new discovery in the Southwest, and printing imaginary geological maps of the United States, of persistently misquoting other writers, of accusing every one of paleontologic or stratigraphic incompetency, and of indulging in personal abuse and vituperation, that Professor Dana at one time demanded that the Boston Society of Natural History should investigate him, and in later years ignored him entirely.

So far as I am aware, his conclusions on the subjects discussed are not accepted by a single living geologist in this country or abroad, and he has hurled criticisms, similar to those now made against me, at the head of every prominent American geologist who has lived since he first came to this country. During the first few years of my studies I was inclined to believe that he might have been right in his conclusions concerning the Jurassic age of the beds of the Tucumcarri region, and committed this opinion to print. So long as I leaned to his opinions he was fulsome in his praise, bestowing upon me effusive compliments as to my ability, etc., etc., and even writing among others of a similar flattering nature, the following notice* of

* Jura Neocomian and Chalk of Arkansas.—Marcou. From the American Geologist, December, 1889, pp. 366–367.

the same Arkansas Report which, in the September number of this Journal, he so severely condemns:

"On the whole, Volume II of the Arkansas geological report for 1888 is a most creditable work, which reflects honor not only on its author, Professor Robert T. Hill, of the University of Texas, by far the best practical geologist who has ever studied Southern Arkansas and Texas, but also on Professor John C. Branner, the state geologist. The State of Arkansas must be complimented to have secured the services of such able observers."

In the American Geologist for September, 1889, p. 156, he also gives me credit as being "The only American Geologist who has quoted my Mesozoic fossils of Texas."* Later, however, after my second visit to Tucumcarri Mesa, where and when I was the first to discover and there to announce the well-developed Cretaceous fauna identical with that of the uppermost Lower Cretaceous beds of Denison, Texas, he immediately directed his epithets at me, in articles elsewhere cited, in the American Geologist, the Proceedings of the Boston Society of Natural History, Science, and this Journal, each attack being proportionately more personal and bitter, as increasing research more and more conclusively demonstrated the Cretaceous age of his New Mexican "Jurassic," and its stratigraphic position above the beds of Comet Creek which he, himself, had called Cretaceous.

I will admit that in the earlier years of my researches, when my papers were largely written in the field away from libraries, I have made occasional mistakes, (and who has not?) some of which are typographic, others slips of the pen, and others merely mistakes, but these papers have always been conscientiously written with a desire to state the truth, and in every instance have been of material advancement to our knowledge of the stratigraphy of the Texas region, and Mr. Marcou's insinuation, "not to use a stronger word," that I have endeavored to corrupt the record is false. No amount of abuse, misrepresentation or misquotation on the part of Professor Marcou can alter the essential facts of research, nor cover up his own misstatement of fact and imperfect and misleading quotations. Even though he should succeed in his attempts to prove me untruthful and a defacer of the geologic record—which he cannot do—this would in no way excuse him for distorting and imperfectly quoting every new scientific discovery in order to uphold an erroneous and untenable deduction, founded on self-confessed incomplete exploration.

* In a subsequent paper in the American Geologist for August, 1894, p 98, in which he makes his first change of front towards me, it is interesting to note that his principal unspecified allegation against me is that I do "not always give credit where credit is due."

Finally, concerning his American geologic work, it can be said now, as was truthfully said by Professor Dana many years ago, that* "We cannot see, therefore, that Mr. Marcou's claims as a discoverer are in any one case sustained, or that his merits are in any respect enhanced by his American researches, and we certainly should not go to him for an exposition of American geology."† . . . "We cannot therefore think that his former reviewers and opponents deserve, because they differ from him, either to have their names expunged from American geological history, or thrown into discredit; nor do we believe that their reputations will seriously suffer from our ambitious Rocky Mountain explorer.‡ . . . Whoever may identify true Permian, true Triassic or true Jurassic strata will not have borrowed from Mr. Marcou and can owe him no credit."§

*This Journal, Nov. 1858, and January 1859. † Ibid., January 1859, p. 139.
‡ Ibid., November 1858, p. 333. § Ibid., November 1858, p. 331.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On Thermal Phenomena attending Change in Rotatory Power.*—It is well known that certain carbohydrates, at the moment of solution in water, show an abnormal circular polarization; and that optical stability in such solutions, at ordinary temperatures, is attained only very slowly. This phenomenon, which was first observed in dextrose and milk sugar, was called *birotation*, since it was believed that the circular polarization of the freshly prepared solution was twice that of the rotation finally reached. Subsequent investigations, however, have shown that even in these cases the ratio of the rotations is never exactly 2:1, and further, that dextrose and milk sugar are capable of existing transitorily in solution in more than two optical conditions; and this, coupled with the fact that the rotatory power of a freshly made solution of maltose is actually less than its subsequent value, has caused the term *multi-rotation* to be adopted as preferable. Now it has been observed that when a substance capable of showing this property is originally produced by the hydrolysis of another carbohydrate of greater molecular complexity, the phenomenon of multi-rotation also appears. And Brown and Pickering have sought to ascertain whether this change in optical properties is attended by any corresponding heat change. Three optically different forms of dextrose have been described by Tanret. His α-dextrose is the so-called birotatory substance, β-dextrose being the ordinary optically stable form. His γ-dextrose, produced by heating amorphous dextrose to 100°–110°, has an abnormally low rotation when freshly dissolved. Since dextrose is produced by hydrolysis in the α form, the authors determined the heat change when this form was converted into β by adding 0·01 per cent of soda solution, the operation being conducted in a suitable calorimeter, and a check determination being made by using the dextrose in a second experiment, already in the β form. The corrected mean of four values is given as 0·588 calory per gram of substance. Plotting the heat changes thus obtained after various intervals of time and comparing the curve with that given by Parcus and Tollens for the rate of change of rotatory power, it appears that, while the heat curve is somewhat flatter, the general character of the two curves is the same. Thus making it probable that "the special physical or chemical changes which determine the gradual alteration or specific rotation in freshly prepared dextrose solutions proceed *pari passu* with the heat evolution," and therefore "that this evolution is the result of the changes in question." Similar experiments were made with maltose, with levulose and with milk sugar. No change of temperature was detected with the first, while levulose gave −4·64 calories per gram and milk sugar

+0·19 calories per gram. As to the cause of multi-rotation, the authors conclude that it is probably "an effect of chemical change brought about by the interaction of sugar and its solvent."—*J. Chem. Soc.*, lxxi, 756–783, July, 1897. G. F. B.

2. *On the Chemical Action of Electrical Oscillations.*—The chemical action of an oscillating electric field upon various substances has been studied by De Hemptinne. His apparatus, which was a modification of that of Lecher, consisted of a pair of plate condensers, one side of each being connected with a Wimshurst machine, driven at a constant speed by a gas motor, an adjustable spark gap being placed between the two plates. The other coatings were connected to two wires of considerable length, terminating in two plates facing each other. Substances placed in the space between these plates are subjected to the influence of the oscillating discharge of the condensers, whose frequency and pressure may be varied at will. If a wire be placed across the two conductors at certain points, an illuminated vacuum tube between the plates becomes dark; while on shifting this wire along, points are reached at intervals where the tube again glows. The distances between these points, as Wiedemann and Ebert have shown, represent the wave length of the oscillation. When very high pressure was needed the author used a Tesla apparatus, the current of an alternating machine being transformed up one hundred fold. The secondary of this transformer was connected to a condenser, and, through a spark gap, with the primary of an induction coil; thus giving in the secondary of this coil, whose terminals ended in two plates opposite to one another, an oscillating discharge of high frequency and pressure. The gas to be examined was contained in a glass cylinder 4^{cm} broad and $12–13^{cm}$ long, having taps at its ends, the lower one terminating in a smaller and graduated tube dipping under mercury; so that the pressure in the cylinder was less than that of the atmosphere. It was observed that no action took place within the tube unless it became luminous. Moreover two tubes may screen each other; and if the pressure of the gas in the tubes be slightly different, then when they are placed between the plates and the discharge is so adjusted that only one glows, a slight increase of pressure in this tube causes it to become dark while the other one glows and is decomposed. The substances exposed to the action of the discharge were ammonia, carbon disulphide, glycerin, oxalic acid and calcium carbonate. It appeared that the speed of decomposition increased with increasing pressure, these pressures being 5, 15 and 50^{mm}; a maximum being reached soon after the decomposition began and then decreased. Moreover this speed is decidedly influenced by the energy of the discharge. The amount of the ammonia eventually decomposed varies with the pressure, being about 50 per cent at 49^{mm} and 95 per cent at 20^{mm}, though the values obtained do not agree with the expression $p, p_2^3/p_3^2 = k$, which theory gives for dissociation by heat. Addition of nitrogen or hydrogen lowers the

decomposition, the former less than the latter. Mixed nitrogen and hydrogen when exposed to electric oscillations unite to the extent of 3 or 4 per cent, the result being apparently independent of the pressure. Carbon disulphide suffers decomposition in the oscillating field, the speed corresponding closely to the equation $dx/dt=k\ (a-x)$. Glycerin and oxalic acid show an increase of vapor-pressure in the oscillating field, but calcium carbonate appears unaffected.—*Zeitschr. phys. Chem.*, xxii, 360–372, April, 1897. G. F. B.

3. *On the Explosion of Chlorine Peroxide with Carbon Monoxide.*—Experiments made a year or more ago by DIXON showed that when a mixture of well-dried carbon monoxide with other burning gases, such as cyanogen or carbon disulphide, was exploded, the carbon monoxide was not completely burned although there was more than enough oxygen for the combustion of both gases and the flame traversed the entire mixture. A suggestion of L. Meyer, that this result was due to the high stability of the oxygen molecule, was disproved by exploding a mixture of well-dried carbon monoxide, oxygen and ozone; which showed that a mixture containing 36 per cent of carbon monoxide and 8 per cent of ozone cannot be fired by a powerful electric spark. Moreover cyanogen is entirely burned to carbon dioxide when exploded with an excess of oxygen. And Smithells, in his flame separator, found that in the inner cone cyanogen is burned to carbon monoxide, this latter being burned completely in dried air when the outer and inner cones are close together, but being extinguished when they are considerably separated. Since it appears, therefore, that carbon monoxide is readily oxidizable when first formed, the author in conjunction with RUSSELL has examined the question whether oxygen when freshly produced would show the same activity toward carbon monoxide. For this purpose they exploded a well-dried mixture of carbon monoxide and chlorine peroxide. Since dry chlorine peroxide yields chlorine and oxygen when detonated, even with an inert gas, it is evident when it is fired in presence of carbon monoxide, this latter gas finds itself intimately mixed with highly heated "nascent" oxygen. The chlorine peroxide was prepared by the action of sulphuric acid diluted one-half with water, upon finely divided potassium chlorate, on a water bath; this gas as well as the carbon monoxide being allowed to pass simultaneously into a specially constructed eudiometer containing phosphoric oxide. The mixture was composed of 29 per cent of chlorine peroxide, 60 per cent of carbon monoxide and 11 per cent of oxygen. After drying for six days, a spark was passed and a pale blue flame traversed the tube. Analysis showed that the residue contained 29 per cent of carbon monoxide, nearly 50 per cent having remained unburned. After decanting the gas and allowing it to stand over potash solution, a spark exploded it violently. A second experiment resulted similarly. The authors therefore do not find "that oxygen just liberated from a compound is more active than

ordinary oxygen in attracting carbon monoxide at a high temperature."—*J. Chem. Soc.*, lxxi, 605–7, June, 1897. G. F. B.

4. *On the Absorption of Nitrogen by Carbon compounds under the influence of the Silent Electric Discharge.*—Experiments have been made by BERTHELOT in further elucidation of the phenomena which result when nitrogen is absorbed by benzene and similar bodies under the influence of the silent electric discharge. He finds that the rate of absorption is more rapid the more frequent the vibrations of the interrupter, a Marcel–Deprez contact-breaker giving better results than the Foucault interrupter even with or without specially high frequency. In the case of benzene he finds the maximum quantity of nitrogen absorbed is about 12 per cent of the mass of the benzene, the absorption being complete in presence of an excess of liquid. If the nitrogen be atmospheric, the absorption is not complete, though the residue is smaller than the quantity of argon present in the air, since a portion of the argon is absorbed. When the absorption is complete the ratio of benzene to nitrogen is $(C_6H_6)_2 : N_2$. The product in its empirical composition corresponds with diphenylphenylenediamine, and shows many properties common to diamines of this class, mixed with condensation products. When exposed to air or oxygen it readily oxidizes, though without setting free nitrogen; and when treated with hydrochloric acid yields salts the bases of which have odors recalling those of quinoline and the hydropyridines. When heated this hydrochloric acid product yields ammonium chloride. The original product when heated by itself gives off large quantities of ammonia, together with benzene, water (resulting from oxidation in the air), a trace of aniline and a bituminous liquid containing nitrogen. Heated in absence of air, it yields ammonia but no free nitrogen. When carbon disulphide is employed, the absorption is more rapid and is complete if the disulphide be in excess. The maximum quantity absorbed is 11·7 per cent of the mass of the bisulphide, the ratio being $(CS_2)_3 : N_2$ the same as with benzene. The product oxidizes in presence of oxygen, no nitrogen being set free. When heated out of contact with the air, some nitrogen is evolved, the larger part remaining in combination with the products of condensation, Hence these products are more stable than those formed by argon or helium. Thiophene under similar conditions absorbs about 8·6 per cent of its weight of nitrogen, the ratio in this case being $(C_4SH_4)_2 : N$.—*C. R.*, cxxiv, 528–532, March, 1897.

G. F. B.

5. *Manual of Qualitative Analysis.* By the late Dr. C. REMIGIUS FRESENIUS. Authorized Translation by HORACE L. WELLS, M.A. New Edition, thoroughly revised, from the Sixteenth German Edition, 8vo, pp. xviii, 748. New York, 1897 (John Wiley & Sons).—The Manuals of Fresenius have stood first in the world among books treating of Analytical Chemistry for nearly fifty years. From the notes on Qualitative Analysis, given to the press in 1841 while he was yet a student at Bonn, down to the

sixteenth edition of this admirable work issued as late as 1895, the author devoted himself untiringly to its improvement, never admitting anything into it without personal verification. It is now fourteen years since the last American edition was published; and since then two thoroughly revised German editions have appeared. By a piece of good fortune Professor Wells was induced to undertake the translation of the book for a new American edition, and good evidence of the accurate and painstaking care with which he has done his work is to be found throughout its pages. While following the original closely and therefore retaining the general form which has given to this Manual its wide reputation, the translator has been obliged to rewrite a large part of the previous edition and to have the whole work reset. In its present form consequently it represents the most accurate methods and the most recent results in qualitative analysis, especially in the chemistry of the rarer elements and the less commonly occurring compounds. No distinction, we notice, is made between the alkaloid termination and that of the glucoside; morphin and salicin terminating alike in *in*. The book will be warmly welcomed by American analysts not only for the great excellence of the original but also for the faithfulness with which it has been put into its English form. G. F. B.

6. *The delay in spark discharges.*—E. WARBURG concludes from his researches that in the ordinary spark discharge the air changes from a very good insulator into a relatively good conductor. In the delay period there is formed, under the influence of the electrostatic force, a very weak invisible electrical current which at the end of the delay period goes over into a visible spark discharge. The delay period continues a longer or shorter time according to the conditions of the electrode; whether they are moist or dry, whether they are under the influence of radiations (ultra-violet radiations—X-ray radiations) or not.—*Wied. Ann.*, No. 11, 1897, pp 385–395. J. T.

7. *Photoelectric relations of Fluorspar and of Selenium.*—Shortly after Hertz had discovered that ultra-violet light lowered the spark potential, E. Wiedemann and H. Ebert showed that this working of light was limited to the cathode. In regard to this phenomenon Prof. J. J. Thomson quotes a hypothesis of Helmholtz that different substances may possess the power of attracting electricity with different intensities—for instance, if a metal draws to itself positive electricity stronger than the surrounding dielectric does then the metal strives to charge itself positively. If the conductor is surrounded by air in its normal condition then it cannot charge itself, since no electricity can escape. All of these conditions are changed when the conductors are subjected to ultra-violet rays. Then enter the following: 1, separation of metallic particles from the conductors; 2, chemical changes in the gas in the neighborhood of the conductor, which break up the gas in such a manner that it can take a charge. According to the above

hypothesis Professor J. J. Thomson explains why metals which charge positively the strongest, also have the greatest sympathy for positive electricity and destroy negative charges to the greatest extent. It has been known that many spots on cubes of fluorspar become negatively charged by light. These, according to the above hypothesis, must possess a greater attraction for negative electricity than for positive, and it would be expected that under the influence of light a positive charge on them would be dissipated. G. C. SCHMIDT has undertaken a research to settle this point and he concludes that fluorspar always charges itself positively at the corners of the crystal and especially on fresh cleavages, and always negatively in the middle. At these places, which are strongest electrified positively in light, the negative electricity is the most quickly dissipated. The positions on fluorspar which are negatively electrified by light are also deprived of negative electricity. Selenium behaves in a similar manner, and the phenomena that bodies charge themselves under the influence of light and dissipate negative electricity are separate phenomena, which are not so closely related as has been supposed. The theory supported by Prof. J. J. Thomson, on the working of light on non-electrified and on negatively charged bodies, is not confirmed by this investigation.— *Wied. Ann.*, No. 11, 407–414.

J. T.

8. *On a magnetic method of showing metallic iron.*—William Duane has shown that the damping effect of a steady magnetic field on oscillating bodies is more delicate than any chemical analysis for the detection of traces of iron. He has extended his investigation to the effect of rotating magnetic fields and finds that in this case the method is even more sensitive that that formerly employed with the steady field.— *Wied. Ann.*, No. 11, 1897, p. 543. J. T.

9. *On the spectra of certain stars.*—A series of observations have been carried on at Potsdam by VOGEL and WILSING having as their object the classification according to their spectra of stars falling in the first spectral class. In the case of one hundred stars the presence of cleveite gas in their atmosphere could be established (Class I*b*); they formed a fourth part of all the stars of Class I which were observed.—*Sitzungsberichte d. K. Preuss. Akad. der Wiss.*, Berlin, Oct. 21.

10. *On the structure of the Cathode Light and the nature of the Lenard Rays.*—GOLDSTEIN shows that the so-called third layer of the cathode light consists of rays in straight lines which have their origin not on the surface of the cathode itself but from the rays of the second layer. The author explains the Lenard rays as due to diffusely reflected cathode rays.—*Sitzberichte d. K. Akad. Wiss.*, Berlin, Oct. 21.

11. *A new Nicol Prism.*—It is announced that C. Leiss has devised a new form of prism of Iceland spar and glass which permits of a saving of 50 per cent in the material.—*Sitz. d. K. Akad.*, Berlin, Oct. 21.

II. Geology and Natural History.

1. *Observations on Baffinland.*—Dr. Robert Bell, F.R.S., of the Geological Survey of Canada, has recently returned from a five months trip into the northern regions. Having proceeded by the Dominion Government S. S. Diana to Hudson Strait, he took a yacht and small boat from there, and made a topographical and geological survey of about 300 miles of the southern coast of Baffinland. From an account in the "Ottawa Citizen" of October 27th we extract the following:

"The northern side of Hudson Strait is the southern coast of Baffinland, which is the third largest island in the world, being 1,100 miles in length. The island of Greenland and the island of Australia alone exceed it in length. Hudson Strait is about 500 miles in length, and averages about 100 miles in width. Big Island, which lies near the north side, is 30 miles long and about 20 miles wide. Both shores of the strait are mountainous, and destitute of trees, being beyond the limits of the northern forests. The land along the western half of the south shore is higher and bolder than any other part of these coasts, and rises to a height of between 1,000 and 2,000 feet above the sea. The eastern half of the south coast is rather low, and is not broken by any mountain range. Dr. Bell says: 'The whole north shore is rugged, but it rises more gradually as we go back from the sea, and attains a general elevation of 1,000 to 1,500 feet at a distance of 10 to 20 miles inland, although some parts are higher; between Frobisher Bay and the eastern part of Hudson Strait, Grinnell glacier, an extensive sheet of ice covers this range, and may be seen from a long distance on a clear day, although one may often pass through Hudson Strait without observing it. In the spring a cold, ice-laden current, flowing in from Davis Strait, passes up the north side of Hudson Strait, while the warmer water of Hudson Bay flows out along the south shore. The north side has therefore a more Arctic character than the south.'"

"On the 20th of July, when Dr. Bell commenced his exploration, the south shore was comparatively free, while the north side had a cold, forbidding appearance. The 'ice-foot,' 20 to 30 feet thick, still adhered to the rocks, all along, except in some of the inlets. At the outset of his journey Dr. Bell was fortunate enough to find an Eskimo who knew the coast and at the same time understood English pretty well, having picked it up at Spicer's Trading Station, which had been maintained in this vicinity for several years.

"The greater part of the coast to be explored was so completely unknown that it was not indicated on the charts even by a dotted line. Passing to the northwest of Big Island, the mainland soon became fringed with many islands, and a little farther on the whole coast seemed to be broken up into innumerable mountainous islands of all sizes, from single hills of rock surrounded by water, to ranges several miles in length. This border of islands

would be from 10 to 20 miles in width. Indeed, it was difficult to know when the mainland was finally reached. Even then, when the explorers ascended a mountain they could see many channels of the sea running in all directions among the high hills. Besides the channels and those separating the islands, a number of large fjords running far inland were discovered and explored. These geographical conditions are due to the geological structure of a great development of crystalline rocks, their extensive erosion and partial submergence. Dr. Bell does not recall such a striking example of this kind of topography in any other part of the world. The northeast coast of Georgian Bay resembles it in some respects on a small scale, but here glaciation has reduced the general surface to a low level and a comparatively even outline."

He also explored inland at one point sufficiently far to locate two great lakes whose southern shores came within 50 or 60 miles of Hudson Strait. The party returned to St. John, N. B., in the latter part of October. H. S. W.

2. *International Geological Congress.*—The International Geological Congress for 1897 took place at St. Petersburg from Aug. 28th to Oct. 5th and in point of view of attendance was the most successful yet held, several hundred geologists from all parts of the world being present and taking part. The session was opened by the honorary president, the Grand-Duke Constantine, and those portions of each day which were devoted to deliberations were divided among the different fields of geologic activity. It cannot be said that any large result followed immediately from these conferences but great moderation and caution were shown by the delegates and the exchange of views will undoubtedly be beneficial to geology in the future. The discussion of the question concerning stratigraphical nomenclature and classification resulted in a general understanding that it was yet too soon to decisively act upon this subject and in the meantime the historical method is recommended as being the most proper ground. In petrography practically the same result was arrived at, the petrographers declining to commit themselves at present to any definite classification but expressing the view that the science had progressed far enough at present to warrant the introduction of some simple group names to be used by the field geologist, and in mapping. The Congress recommended the establishment of an international station for the investigation of the sea bottom.

Dr. Hauchecorne and Dr. Beyschlag were placed in charge of the commission for the geological map of Europe. The invitation of the French geologists was accepted and Paris named as the place of meeting in 1900. A committee was appointed to report on the advisability and possibility of the establishment of an international journal devoted to the interests of petrography, especially for reviews.

A number of interesting exhibits were shown, among them that by the Imperial Geological Survey of Japan being, perhaps, the one that attracted the most attention.

A considerable number of American geologists were present, among them the venerable but active Prof. James Hall of Albany. The following were named vice-presidents: Prof. O. C. Marsh, Prof. B. K. Emerson, Mr. S. F. Emmons, Dr. Persifor Fraser.

The excursions both before and after the Congress were well attended and were managed with great ability and success considering the obstacles which in many cases had to be overcome in moving so large a number of people from point to point. Here the influence of the Russian government was clearly perceptible. Before the Congress the excursions were in Finland, Esthonia and to the Urals, and after it the excursion presented a choice of routes to the Caucasus and then through to Asia Minor and on the Black Sea, including the Crimea. It lasted upwards of a month. For these excursions, the members of the Congress were indebted to the Russian and Finnish governments for free transportation.

It would be well if in the future some plan could be devised by which these excursions could be strictly limited to those whom they are designed to benefit, and the alleged scientists, who join them for the sake of obtaining the advantage of cheap travel, could be cut off. L. V. P.

3. *Mineral Resources of the United States*, 1895, DAVID T. DAY, Chief of Division (17th Ann. Report of the U. S. Geological Survey, Charles D. Walcott, Director). Washington, 1896.—The appearance of the seventeenth annual report of the United States Geological Survey has already been noticed in this Journal, but the two volumes on Mineral Resources of the United States in 1895 call for an additional remark. These volumes together form Part III of the complete report. This is the second time that the work on the mineral resources brought out by Mr. David T. Day has been published in this form.

The first of the two volumes is devoted to the metallic products and coal, the second to non-metallic products. They contain a valuable series of papers by various authors, of which the following may be mentioned among others: on iron ores by John Birkinbine; on copper and on lead by Charles Kirchhoff; on manganese, on coke and on petroleum by Joseph H. Weeks; on coal by Edward W. Parker; on stone by William C. Day. An interesting chapter by George F. Kunz is devoted to the precious stones of the country.

4. *A Descriptive Catalogue of Useful Fiber Plants of the World, including the structural and economic classifications of fibers;* by CHARLES RICHARDS DODGE, Special Agent, U. S. Department Agriculture. Washington, 1897.—It is doubtful whether the general public realizes the extent to which investigations bearing on economic development have been carried on, of late years, at the instance of the departments. The treatises are numerous and, for the most part, of excellent quality. The one before us is a case in point. Mr. Dodge has brought together in this convenient form a vast amount of information, much of

which he has apparently verified with his specimens in hand. The range of authorities laid under contribution is wide and has been thoroughly traversed by the author. We naturally expected to see in his list WIESNER'S *Die Rohstoffe des Pflanzenreiches*, and BARON VON MUELLER'S *Extratropical Plants*, but the other omissions which have come to notice are slight and unimportant. It seems a pity that this valuable compendium could not have been enriched by plates of the fibers themselves both in their commercial and ultimate reductions. But, even as it stands, it will set many a cultivator thinking in what way new fiber-plants can be obtained for experiment here. G. L. G.

II. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *National Academy of Sciences.*—The autumn meeting of the National Academy of Sciences was held at Boston, beginning November 16. The following is a list of the papers accepted for reading:

R. S. WOODWARD: The mass of the earth's atmosphere.

W. A. ROGERS: On a final determination of the relative lengths of the Imperia yard and of the meter of the International Bureau.

C. BARUS: The secular softening of cold hard steel.

T. C. MENDENHALL: On the elastic resistance of steel knife-edges

A. HYATT: Evolution and migrations of land shells on Hawaiian islands.

R. H. CHITTENDEN: The influence of borax and boric acid on nutrition.

C. S. MINOT: Embryological observations.

IRA REMSEN: On a new method of obtaining derivatives of guanidine; On the boiling points of mixtures of benzine and alcohols; On double halides containing organic bases

A. A. MICHELSON and S. W. STRATTON: Results obtained with a new harmonic analyzer.

E. S. MORSE: On the ancient molluscan fauna of New England.

C. R. CROSS: On a new application of the wave siren.

C. L. NORTON: New apparatus for the comparison of thermometers and for the determinations of the heat of combustion of fuels.

O. C MARSH: Recent observations on European Dinosaurs; The Jurassic formation of the Atlantic coast—Supplement

A. E. VERRILL: Ovarian variations and cannibalistic selection as factors in the evolution of species; Notable instances of free variation nearly unchecked by natural selection; Some of the important factors in the evolution of the marine animals of coral-reef seas.

S. C. CHANDLER: Comparison of the theory of the motion of the pole with recent observations.

J. W. POWELL: An hypothesis to account for movements in the crust of the earth.

S. WEIR MITCHELL and ALONZO H. STEWART: A contribution to the study of the action of the venom of the *Crotalus adamanteus* upon the blood.

S. F. EMMONS: Report on the international geological congress at St. Petersburg in August, 1897.

A lecture was delivered by JOHN TROWBRIDGE at the Jeffersonian Physical Laboratory, Cambridge, on electrical discharges, with exhibition of apparatus for obtaining high voltages

At the business meeting of the Academy on Nov. 17, it was announced that Miss Alice Bache Gould, daughter of the late Benjamin Apthorp Gould, had presented a sum of $20,000, to be known

as the Benjamin Apthorp Gould fund, the proceeds of the fund to be used at the discretion of a board of three directors, two of whom must be members of the Academy, in furthering astronomical research. This fund was later formally accepted by the Academy; the following directors were appointed by Miss Gould: Prof. Lewis Boss of Albany, Dr. Seth C. Chandler of Cambridge and Prof. Asaph Hall of Washington.

2. *Cordoba Photographs:* Photographic Observations of Star-Clusters, from impressions made at the Argentine National Observatory, measured and computed by BENJAMIN APTHORP GOULD, Lynn, Mass., 1897.—The growing importance of astronomical photography is manifested in this extensive contribution to the subject, which completes the long array of results derived by Dr. Gould from his southern sojourn. It contains the results of the measurement of 177 plates, taken at Cordoba with an equatorial of 11¼ inches aperture, of which the original objective was that first devised and used by Rutherfurd, but having been broken on the journey to South America was replaced by a similar one by the same maker, Mr. Fitz. The objects photographed were mainly star-clusters and richer portions of the southern hemisphere, 37 districts being included in the present discussion, and some 27 yet remain to be computed. In all, the positions of 9144 stars are furnished, referred to 78 centers by polar coördinates; these are then converted into differences of right ascension and declination. For each plate four constants are determined by reference to known star-places, these being mainly derived from the Cordoba meridian observations. The four constants determined for each plate are the corrections to the rectangular coördinates of the origin and to the adopted scale-value and position-angle zero. This method seems amply adequate for the degree of accuracy aimed at, and the work will doubtless prove a most valuable addition to our knowledge of the southern heavens.

The volume is edited by Dr. S. C. Chandler, to whom this duty was confided after the death of Dr. Gould, who most lamentably was not to see the finished work, though the computations and discussion had all but completely passed through his hands.

W. L. E.

3. *November Meteors, 1897.*—A watch was kept at the Yale Observatory on the night of Saturday, Nov. 13, for 6 hours commencing at 11 p.m., by Mr. Brown (for one half of the time) and Mr. Smith, who exposed plates in the photographic apparatus. In all 30 meteors were seen during these hours, only 5 of which were conformable to the Leonid radiant. Only one of these fell in the area covered by the cameras and this was not bright enough to impress on the plates, which were much fogged by the moon, then only 4½ days past full. The nights of Nov. 14, 15 and 16 were completely overcast here at New Haven. W. L. E.

4. *Sixteenth Annual Report of the Bureau of American Ethnology* to the Secretary of the Smithsonian Institution, 1894–95; by J. W. POWELL, Director, 326 pp., with 81 plates and 83 figures in the text. Washington, 1897.—The sixteenth annual

report has been recently distributed to the public, and like those which have preceded, it gives evidence of the activity in this department and of the excellent work that is being done in the study of the many ethnological problems of this country. After the administrative report of the director, the following papers are given: Primitive Trephining in Peru, by M. A. Muñiz and W. J. McGee; Cliff Ruins of Canyon de Chelly, Arizona, by C. Mindeleff; Day Symobols of the Maya Year, by Cyrus Thomas; Tusayan Snake Ceremonies, by J. W. Fewkes.

5. *Field Columbian Museum.*—Recent publications of the Field Columbian Museum at Chicago include the following:

Publication 16. Anthropological Series. Vol. i, No. 1. Archeological Studies among the Ancient Cities of Mexico. Part II, Monuments of Chiapas, Oaxaca and the Valley of Mexico. By William H. Holmes. 338 pp. This is a valuable contribution to a highly interesting subject. It is profusely illustrated, containing with other plates numerous panoramic views which the author has drawn with his well-known skill.

Publications 19 and 20. Zoölogical Series. Vol. i, Nos. 6 and 7. List of Mammals from Somali-Land obtained by the Museum's East African Expedition, and Remarks upon two Species of Deer of the Genus Cervus, from the Philippine Archipelago, by D. G. Elliot, F.R.S.E. 155 pp. (Plates.)

Publication 21. Anthropological Series. Vol. ii, No. 1. Observations on a Collection of Papuan Crania, by George A. Dorsey. With Notes on Preservation and Decorative Features, by William H. Holmes. 49 pp.

6. *The American Journal of Physiology.*—Attention is called to the following circular, which gives the prospectus of a new journal in a field not yet occupied in this country. It deserves the hearty support of all interested in the department.

The number of investigations in physiology and its allied sciences now made in this country is grown so large that the present means of publication are no longer sufficient. To meet the needs of investigators in physiology, physiological chemistry, physiological pharmacology, and certain other branches of biology, a special journal will be published, the first number appearing in January, 1898. The American Journal of Physiology, as the new publication will be called, will contain in each volume about five hundred pages, divided into parts or numbers, to be issued whenever material is received. It is expected that not more than one volume a year will be printed. The Journal will be edited for the American Physiological Society by H. P. Bowditch, M.D., Boston; R. H. Chittenden, Ph.D., New Haven; W. H. Howell, M.D., Baltimore; Frederic S. Lee, Ph.D., New York; Jacques Loeb, M.D., Chicago; W. P. Lombard, M.D., Ann Arbor; and W. T. Porter, M.D., Boston.

It is not to be supposed that a journal devoted solely to the publication of original researches in physiology will ever do more than pay for its paper and printing, and it is probable that some years must pass before the new enterprise will cease to be a financial burden on a small number of investigators. Yet the need of such a publication is undoubted. The aid of all friends of learning is asked until the Journal shall be established on a self-supporting basis. The subscription price, which is five dollars (£1 1*s.*; marks, 21; francs, 26) per volume, should be sent to W. T. Porter, M.D., 688 Boylston Street, Boston, Mass.

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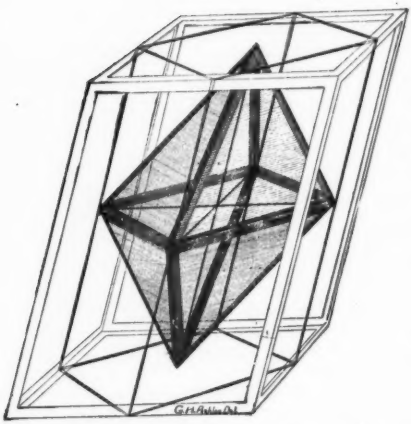

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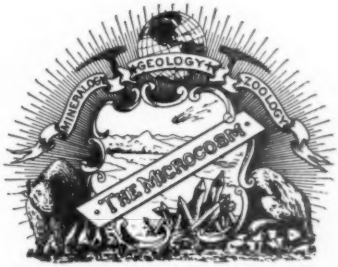

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Wenn ich nun wieder statt des zweiten Theiles der Gastheorie den ersten der Mechanik publicire, so will ich das nicht mit dem Hinweise auf berühmte Muster für die Reihenfolge des Erscheinens der Bände entschuldigen. Das kam vielmehr so. Im zweiten Theile der Gastheorie häuften sich die nothwendigen Einschaltungen über Mechanik so, dass sie erst einen ganzen Paragraph, dann einen Abschnitt auszufüllen schienen und ich mich zuletzt entschloss, ein ganzes Buch daraus zu machen, indem ich noch ein Vorlesungsheft hinzunahm, das ich in den vorigen Ferien für Vorlesungen über Mechanik im folgenden Wintersemester ausgearbeitet hatte. Als ich mir aber mein Auditorium betrachtete, glaubte ich die ganze Methode desselben mit einer einfacheren vertauschen zu sollen. Dafür nahm ich den Inhalt des betreffenden Heftes, damit doch meine Mühe nicht ganz verloren sei, in das vorliegende Buch auf, welches ich also als Vorlesungen bezeichnen könnte, die ich an der Wiener Universität nicht gehalten habe.

Man sprach in neuerer Zeit viel über die Dunkelheiten in den Principien der Mechanik und suchte sie dadurch zu beseitigen, dass man der Mechanik ein ganz neues, fremdartiges Gewand gab. Ich habe hier den entgegengesetzten Weg eingeschlagen und versucht, ob sich nicht bei möglichst treuer Darstellung der Mechanik in ihrer alten classischen Form die Dunkelheiten ebenfalls vermeiden liessen, theils indem ich gewisse Dinge, die man früher überging oder als selbstverständlich nur obenhin berührte, ausführlich behandelte, theils indem ich jede berechtigte Kritik sorgfältig berücksichtigte. Besonders kann ich da den Bemerkungen Hertz' über den Ideenreichthum der einschlägigen Schriften Mach's nur aufs Wärmste zustimmen, wenn ich auch keineswegs überall derselben Ansicht bin, wie Mach.

Gerne hätte ich mich der Bezeichnungsweise der Quaternionen angeschlossen; doch wäre dies meinem Bestreben, alles dem deutschen Publikum fremdartige auszuschliessen, zuwidergelaufen.

Der zweite Theil der Vorlesungen über Mechanik, der erst die gastheoretisch wichtigen Principe bringen wird, soll in kürzester Zeit und dann, sobald es mir möglich sein wird, auch der zweite Theil der Gastheorie erscheinen. Ein dritter

Theil der Mechanik soll die Elasticitätslehre und Hydrodynamik enthalten, und so wieder zur Gastheorie zurückleiten.

Von Vollständigkeit und wesentlich Neuem kann natürlich bei einem so umfangreichen und vielbearbeiteten Gebiete, wie die Mechanik, nicht die Rede sein. Trotzdem stellte es sich vom Abschnitte über das Kräftenparallelogramm (§ 10) bis zur Definition des Gleichgewichtes eines Systems, dem d'Alembert'schen Principe und der allgemeinen Form der Bewegungsgleichungen (§§ 72, 73 und 74) heraus, dass auch manche specielle Sätze der Mechanik noch der schärferen Präcisirung bedürfen. Natürlich konnte hier keine dieser Fragen zum Abschlusse gebracht werden; das wäre nur in einer Monographie möglich; aber ich bin zufrieden, wenn ich auf Lücken hingewiesen und Anregung zu weiterer Forschung gegeben habe.

Abbazia, den 3. August 1897.

Ludwig Boltzmann.

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XII, 139 Seiten mit vielen Textfiguren und 2 lithogr. Tafeln. 1891. Mk. 5.—.

II. Theil:

Verhältniss zur Fernwirkungstheorie, specielle Fälle der Elektrostatik, stationären Strömung und Induction.

VIII, 166 Seiten mit Figuren im Text und 2 Tabellen. 1893. Mk. 5.—.

Stimmen der Presse:

Zeitschr. f. phys. u. chem. Unt.: ... Nur ein Boltzmann konnte den oft unentwirrbar complicirten Plan des Maxwell'schen Lehrgebäudes bis in alle Details so verstehen, um ihn mit dieser Klarheit blosszulegen. Aus den einfachsten Annahmen — den Gesetzen der cyklischen Bewegungen und der Lagrange'schen Gleichung — entwickeln sich die weittragendsten Schlüsse mit einer Klarheit und Eleganz, die neben der vollendeten wissenschaftlichen Befriedigung auch einen hervorragenden ästhetischen Genuss bietet.

Elektrotechn. Zeitschr.: ... Dem Werke eines unserer bedeutendsten mathematischen Physiker wird zweifellos allseitig das grösste Interesse entgegengebracht werden.

... Von allen Versuchen, welche bisher unternommen wurden, um die Maxwell'sche Theorie in folgerechter Entwickelung darzustellen, müssen die Boltzmann'schen Vorlesungen an erster Stelle genannt werden.

Zeitschr. f. d. österr. Gymnas.: ... Die Arbeit ist zweifelsohne unter allen bisher erschienenen die geeignetste, um den Zusammenhang der alten und neuen Elektricitätslehre zu erfassen und das leichtere Studium Maxwell's anzubahnen.

Zeitschr. f. d. Realschulw.: ... Die Fülle schöner Details, die Eleganz der mathematischen Darlegungen, die trotz der grossen Ansprüche, die an den Leser gestellt werden, doch äusserst klare Ausdrucksweise werden dem Buche gewiss viele Freunde sichern.

Deutsche Litteraturztg.: ... Die Darstellung, die sich durch Klarheit, Kürze und Uebersichtlichkeit auszeichnet, lässt die Maxwell'schen Ideen in einem neuen Lichte erscheinen und ist wohl geeignet, uns auch das, was in der Theorie zuerst fremdartig anmuthet, zum Verständniss zu bringen. Das Buch, das erste seiner Art, das in Deutschland veröffentlicht ist, kann daher als eine wesentliche Bereicherung der Litteratur über Maxwell's Theorie bezeichnet werden.

Beibl. d. Annalen d. Phys. u. Ch.: ... Selbst der mit der Maxwell'schen Theorie in ihren verschiedenen Formen schon voll Vertraute wird das originell geschriebene, an Anregungen und an pädagogischen Feinheiten in der Darstellung reiche Buch mit Genuss lesen.

Verlag von Johann Ambrosius Barth in Leipzig.

Vorlesungen über Gastheorie

von

Dr. Ludwig Boltzmann,

Professor der theoretischen Physik an der Universität Wien.

I. Theil:

Theorie der Gase mit einatomigen Molekülen, deren Dimensionen gegen die mittlere Weglänge verschwinden.

IV, 200 Seiten. 1895. Mk. 6.—.

In dem vorliegenden Werke, das aus an der Münchener und Wiener Universität gehaltenen Vorlesungen entstanden ist, versucht der Verfasser vor Allem die bahnbrechenden Arbeiten von Clausius und Maxwell übersichtlich wiederzugeben. Aber auch seinen eigenen Arbeiten ist einiger Platz gegönnt. Eine kurz gefasste, möglichst leichtverständliche Darstellung einiger Hauptresultate der Gastheorie Boltzmann's wird von den Fachgenossen gewiss mit grosser Freude begrüsst werden.

In einem II. Theile sollen die van der Waals'sche Theorie, die Gase mit mehratomigen Molekülen und die Dissociation behandelt werden.

Stimmen der Presse:

Literar. Centralbl.: Der Verfasser, der im Laufe seiner Studien selbst namhafte Beiträge zur Entwickelung der kinetischen Gastheorie geliefert hat, bringt uns eine hochinteressante Bearbeitung des gesammten Gebietes, indem er zugleich vielfach neue Gesichtspunkte darbietet und neues Geschütz ins Feld führt gegen die sich mehrenden Angriffe von Seiten der Energetik. . . . Das Werk ist überall verständlich geschrieben und birgt eine Fülle der schätzbarsten Untersuchungen.

Zeitschr. f. österr. Gymnas.: Wir empfehlen das treffliche Buch den Studirenden, welche bestrebt sind, eine tüchtige Grundlage für den höheren Calcül und die höhere Algebra zu gewinnen und auf leichte Weise in diese einleitenden Partien eingeführt werden wollen.

Elektrotechn. Zeitschr.: . . . Um es kurz zu sagen: das Buch ist ein echtes Produkt Boltzmann'schen Geistes.

Journ. of Phys. Chem.: . . . In conclusion, the student desirous of studying the Kinetic Theorie of Gases can hardly do better than take up this book, confident that he will here find a clear and mathematically sound treatment of the subject.

Gustav Robert Kirchhoff

Gedächtnisrede von **Ludwig Boltzmann.**

32 Seiten mit Kirchhoffs Porträt. 1888. Mark 1.—.

Verlag von Johann Ambrosius Barth in Leipzig.

Vorlesungen über Theoretische Physik

von

H. von Helmholtz.

In sechs Bänden.

Dem zuerst erschienenen Band V:

Vorlesungen

über die

Elektromagnetische Theorie des Lichts

herausgegeben von

Arthur König und **Carl Runge**

XII, 370 Seiten mit 54 Figuren. 1897. Mk. 14.—.

werden zunächst folgen:

Band I Abtheilung 2: **Dynamik discreter Massenpunkte,** herausgegeben von Otto Krigar-Menzel;

Band III: **Mathematische Principien der Akustik,** herausgegeben von Arthur König und Carl Runge;

Band I Abtheilung 1: **Die allgemeinen Grundlagen der physikalischen Wissenschaften,** herausgegeben von Arthur König;

In noch nicht feststehender Reihenfolge sodann:

Band II: **Dynamik continuirlich verbreiteter Massen,** herausgegeben von Otto Krigar-Menzel;

Band IV: **Elektrodynamik und Theorie des Magnetismus,** herausgegeben von Otto Krigar-Menzel;

Band VI: **Theorie der Wärme,** herausgegeben von Franz Richarz.

Mit allseitiger Freude wird es begrüsst werden, dass die Vorlesungen des grossen Gelehrten nun doch noch gedruckt erscheinen werden. Sie sind hochbedeutend, da in ihnen Forschungsresultate niedergelegt sind, die H. anderweitig noch nicht veröffentlicht hatte. Für die Wichtigkeit des zuerst erschienenen Band V mag allein darauf hingewiesen werden, dass eine zusammenfassende Darstellung der Optik auf Grundlage der elektro-magnetischen Lichttheorie überhaupt noch nicht vorhanden ist.

Es ist zu hoffen, dass noch während des Winters 1897/98 2 weitere Bände erscheinen können.

Stimmen der Presse:

Elektrot. Zeitschr.: ... Wir begnügen uns damit, der Genugthuung Ausdruck zu geben, dass H.'s werthvolle Vorlesungen in dieser Weise erhalten bleiben, und wünschen dem Unternehmen guten Fortgang und guten Erfolg.

Zeitschr. f. phys. u. chem. Unter.: ... Der Leser dieses Werkes wird, selbst wenn er mit dem Gegenstand desselben vollkommen vertraut ist, eine Fülle von Anregungen und neuen Gesichtspunkten finden. War es ja doch ein charakteristisches Merkmal der Helmholtzischen Arbeitsmethode, sich stets alles auf seine eigne Weise zurechtzulegen, auf eignen Wegen zu wandeln. ... Die Ausgabe ist nach einem Stenogramm ausgearbeitet. Es ist ein dankenswerther Zug der Pietät, dass die Herausgeber den ursprünglichen Text so viel wie irgend möglich geschont haben. Wer das Glück genossen hat, in späteren Jahen den Meister zu hören oder sich im Gespräch wissenschaftlichen Rath zu holen, wird bei dem Lesen ganzer Seiten des Buches, bei den zahlreichen specifisch Helmholtzischen Wendungen und Ausdrücken in die Illusion versetzt werden, den intellectual giant, wie ihn Maxwell einmal genannt hat, noch einmal zu hören.

Druck von Metzger & Wittig in Leipzig.

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